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ARTICLES

THE REAFFIRMATION OF A MULTIPARTY SYSTEM IN FRANCE

JOSEPH A. SCHLESINGER
MILDRED SCHLESINGER
Michigan State University

In accord with the office-seeking theory of parties, we explore the impact of the structure of electoral competition on French parties. We speculated that the Fifth Republic's electoral structure—dual-ballot elections in single-member districts—would produce a multiparty system consisting of parties tailored to the two-ballot mode of winning. To test our proposition we devised two measures of winning for the members of the national assembly's partisan groups: the percentage of members who won the absolute majority that was needed to win on the first ballot and the average shift in the electoral margin of the groups' remaining members from the first to the second ballot. The two measures revealed four distinct ways of winning, each of which fostered a prototypical party.

The Fifth Republic's most impressive accomplishment is its solution to the problems of political parties that plagued its predecessors. Unlike its predecessors, the Fifth Republic has a party system capable of performing the basic task asked of parties in a democracy—the production of alternative stable governing majorities. Nevertheless, the Republic's party system remains the same as that of its predecessors in one critical aspect; it remains a multiparty system. It is, however, a multiparty system renovated to achieve what the Third and Fourth Republics could not.

To understand the renovation and its consequences we turned to the office-seeking theory of parties. The office-seeking theory assumes that in democracies elective office goals dominate political parties. Within parties, then, elected officeholders and candidates for office are the most important individuals. It follows that parties' organization and behavior will reflect the aspects of a political system that guide politicians' expectations about elective offices and the means for achieving them (J. Schlesinger 1965, 1984). If the Fifth Republic's parties perform better than their predecessors, we must seek the explanation in how politicians' expectations have been altered.

Two aspects of a political system, we feel, are crucial to the organization of political parties. One is the structure of political opportunities. This structure consists of all the elective offices in a political system. It therefore lays out the goals and directs the ambitions of politicians. The other aspect is the structure of electoral competition. This structure lays out how offices can be won. Together, these two factors provide the impetus for the development of party organization (J. Schlesinger 1960, 1965, 1966). Those who would renovate parties and the party system must therefore renovate one or both of these structures. The Fifth Republic altered both structures.

The Renovation of the Structure of Political Opportunities in the Fifth Republic

By redesigning the structure of political opportunities in France, the Fifth Republic's founding fathers clarified the office-seeking base of French parties. During the Fifth Republic the relationship between the maintenance of parties and their chances at gaining important offices has become blatant. The most striking innovation in the structure of opportunities was the establishment of a popularly elected presidency. In response, every significant party of the Fifth Republic has emerged as a national organization, renovated or newly minted for the purpose of serving presidential ambitions (Charlot 1967, 1970; Colard 1983; Colliard 1972, 1979; Duhamel 1980; Ponceyri 1985; Portelli 1980; Schonfeld 1980; Wilson 1982).

Moreover, the newly established office of the presidency imposed more concerted and disciplined action on French parties than in the two previous republics. The president gained formidable powers over the legislature, impelling party organizations to provide the executive with consistently reliable majorities for the first time in French republican history. In response there emerged for the first time a unified opposition with realistic aspirations for providing a reliable governing alternative.

All the same, despite the novelty and importance of the presidency, we must not overlook the continued importance of popularly elected national legislative office in the renovated structure of political opportunities or its consequences for party organization.[1] Despite the considerable power granted to the president over the popularly elected national assembly, the assembly came to play an increasingly important role in the implementation of the president's program. As a result, partisan organization within the assembly has had a critical role to play. Efforts on the part of the president to impose his stamp on legislative elections and their parliamentary outcome have been successful only when the president has identified with the dominant legislative group. Indeed, Converse and Pierce (1986, 552) have called disciplined voting in the assembly the clearest manifestation of partisan organizational behavior.

The Renovation of the Structure of Electoral Competition in the Fifth Republic

Important as the renovation of the structure of political opportunities has been for party organization in France, we wish to emphasize the effect of renovating the structure of *electoral competition*. The structure of competition has a major bearing on the nature of party organization in democracies. The structure of opportunities defines what choices the voter makes. Electoral rules define how the choices are made and how they translate into electoral victory (Grofman and Lijphart 1986; Rae 1967). The most important choice voters make in democracies in electing the officials who govern them; but office seekers must heed the way voters make those choices. Electoral rules then become guidelines for organization.

In two of the three oldest democracies, the United States and Great Britain (the third being France), electoral rules allow the voter only one choice for each office. In these two democracies single candidates win by capturing a plurality of votes in one round of elections. No provision is made for the ranking of preferences. Under these circumstances, many voters will respond sincerely by voting for their first preference among the candidates presented. Others, however, will vote strategically by choosing the candidate they prefer most or dislike least among the candidates they believe have a chance to win.[2] As a result, those organizing to capture office must be sensitive to a whole range of voter preferences. In nominating candidates and taking stands on issues, they cannot rely on the voter's firm allegiance to a first choice. The British and U.S. electoral systems then, encourage the formation of a few broad partisan organizations seeking to balance within themselves the range of choices that the systems deny the voter.[3]

The two-ballot system reintroduced in the Fifth Republic for most important elective offices frames the choices before the electorate differently, with different consequences for political organization.[4] Under this system candidates are chosen in single-member districts in two rounds of elections. Candidates can win on the first ballot by receiving an absolute majority of the valid votes cast. On the second ballot candidates win by receiving a plurality of the votes. French electoral rules therefore allow the voters more options than U.S. and British rules.[5] Knowing that a second ballot will occur if no candidate wins on the first ballot, voters may well choose to vote sincerely on the first ballot, leaving their strategic choice for the second ballot. Or they may underline their sincere choice by abstaining on the second ballot or casting a blank vote. Conversely they may choose to cast what the French call a "useful" vote on the first ballot, passing up their sincere choice to ensure that their second preference is guaranteed a place for the second contest. They may even choose to underline their strategic vote by voting only on the second ballot, after the field has been narrowed.[6]

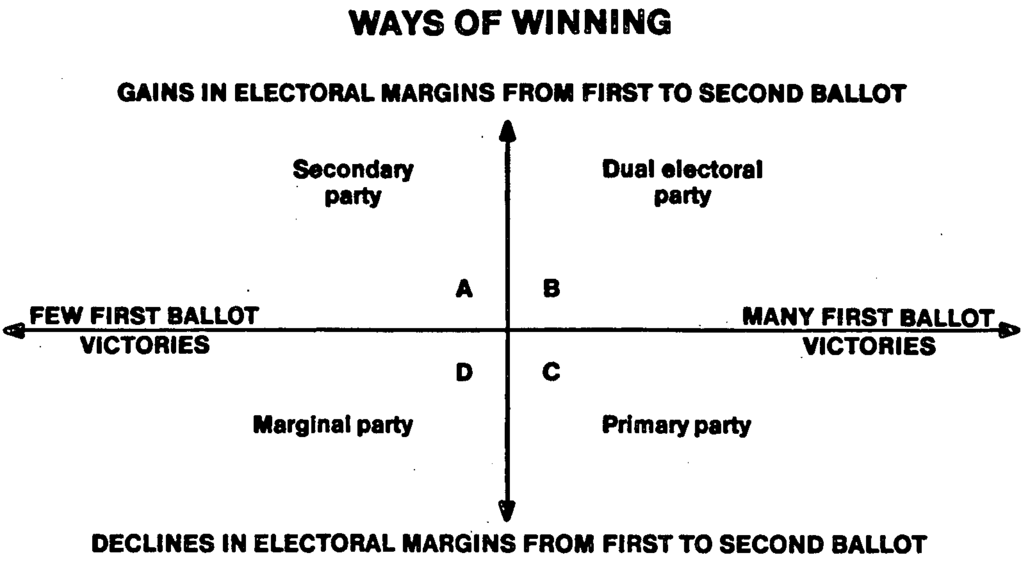
Given the greater options offered the voters in ordering their preferences, it follows that those organizing to capture office will also have more options. The most obvious option is to form more, rather than fewer, organizations to rely on coalitions between, rather than within, organizations. The two-ballot system also encourages other organizational options. One is to create an organization whose candidates and policies will be the voters' first choice. Another is to construct an organization aimed at appealing to voters as a second, or compromise, choice. Still a third is to try to construct an organization aimed at appealing to some voters as a first, and to others as a second, choice—although this is undoubtedly the more difficult organization to construct. Qualities that make a party an attractive first choice for some can make it an unattractive second choice for others.

In effect, the French two-ballot electoral system encourages the organization of parties that specialize. It is true that across the range of constituencies parties can play the game differently depending on their strength. Certainly, variations in constituencies make it unlikely that any party will be exclusively a first- or second-ballot organization. Yet in a parliamentary system there are limits to the degree to which parties can vary their appeal from constituency to constituency, hence the likelihood of some form of verifiable specialization.

A Typology of Parties in a Dual-Ballot Electoral System

We can test our proposition about specialization by examining how legislative groups (which we have singled out as the core of partisan organization) win in the single-member-district, two-ballot arrangement for elections to the national assembly. Since legislators can win on the first or second ballot, we have devised two measures of winning.[7] The most

Figure 1. A Typology of Parties in a Two-Ballot, Single-Member District System of Electing Representatives


WAYS OF WINNING
GAINS IN ELECTORAL MARGINS FROM FIRST TO SECOND BALLOT
Secondary party
Dual electoral party
A
B
FEW FIRST BALLOT VICTORIES
MANY FIRST BALLOT VICTORIES
D
C
Marginal party
Primary party
DECLINES IN ELECTORAL MARGINS FROM FIRST TO SECOND BALLOT

direct measure of a group's success on the first ballot is the percentage of its members who win by taking an absolute majority of the votes.[8] The significant measure of a group's success on the second ballot, where candidates need only a plurality to win, is the shift in the average electoral margin of the group's members from the first to the second ballot. This measure can be positive or negative, since it measures the increase or decrease in a group's voting strength between the two ballots relative to that of its competitors.[9] We stress the relative position because winners on the second ballot always increase their vote, given the withdrawal of candidates due to agreements between parties or to the legal requirement for access to the second ballot.[10]

Together, the two measures reveal four distinct ways of winning, each of which fosters a prototypical party (see Figure 1). Winning mostly on the first ballot or mostly on the second ballot fosters two basic types of organization, the *primary* and the *secondary party*, respectively. Their variants are the *dual electoral party* (which does well on both ballots) and the *marginal party* (which has difficulty winning on either ballot).

The primary party (whose candidates win mostly on the first ballot) is designed to appeal to voters as their first choice. Since first choices are likely to be firmer than second, or compromise, choices, this means creating an organization whose candidates and policies have a strong appeal and whose resources are ample, allowing the organization to maximize its vote on the first ballot. A strong appeal should not be confused with a sharply defined ideological message, which in fact tends to restrict, rather than attract, the broad support needed for many first-ballot victories. Rather, the primary party's appeal is to the basic instincts of a broad spectrum of the electorate, for instance, to the instinct to conserve or maintain stability. A popular individual can personalize this appeal. Thus, the primary party emerges as an organization dominated by a single individual who overshadows all others, making them dependent for election on his drawing power. In turn, mutual dependence forges links among officeholders. The primary party, then, is likely to be a centralized, orderly organization.

The secondary party, whose candidates take most of their victories on the second ballot, is constructed to appeal to voters as their second choice. Note that this does not mean aiming only at second-ballot votes. Recall that voters have the option of voting strategically on the first ballot for a second-choice candidate who, they feel, is more likely to win. Nevertheless, the organization is designed to maximize its support on the second ballot. This means constructing an organization whose candidates and policies are capable of attracting voters with other partisan preferences on both ballots. Flexibility and adaptability, then, characterize this organization. But this is not achieved by putting forth a broad, vague appeal. Rather, it requires targeting different groups of voters with specific appeals without antagonizing other groups whose votes the party also seeks to win. Nor do flexibility and adaptability mean loose organization. On the contrary, the mutual need to attract support beyond the partisan base forges strong links among all those holding and seeking office.

The dual electoral party, whose candidates do well on both ballots, is a variant of the primary party. Many of its candidates win on the first ballot. Those who win on the second ballot win handily (unlike the primary party's candidates). This organization is designed to appeal to some voters as their first choice, and to others as their second choice. Since it attempts both types of appeals, it differs markedly from both the primary and secondary parties. Dual behavior allows for the greatest degree of independence among officeholders. Unlike the primary party, it cannot be overshadowed by a single, dominant personality. Unlike the secondary party, the widespread need for mutual assistance is absent. It is then likely to be a looser, more heterogeneous organization than the other two parties, an umbrella organization encompassing notables or officeholders who operate with considerable freedom in the single-member districts that the electoral system prescribes.

The marginal party, whose candidates have difficulties with both ballots, is a variant of the secondary party. Its candidates have difficulty winning on the first ballot, (like the secondary party's). And though most of its candidates win on the second ballot, they win unimpressively. The marginal party is an organization that many voters are reluctant to accept as either a first or second choice. The most likely reason is a sharply defined message targeted at a limited group of voters. In this it differs from the other prototypical parties. It does not rely on a broad instinctive appeal or a popular personality (as the primary party does). It is unwilling or unable to target multiple groups of voters (as the secondary party does). Unlike the dual electoral party, it is incapable of dual behavior. Thus, we infer a rigid organization, whose officeholders and office seekers are bound and confined together by limited options.

The two-ballot system, which fosters four distinctive ways of winning, also fosters two distinctive strategies of alliance, which derive from the needs of the two basic prototypical parties. These strategies in turn reinforce the primary and secondary parties. As the party designed to be the voters' first choice and win on the first ballot, the primary party favors the strategy of the unique candidacy. This means it uses its resources to try to get likely allies to agree on a single or unique candidate in each constituency before the first ballot. This strategy has the best prospect for amassing the absolute majority of votes needed to win on the first ballot. Faced with a second ballot, however, unique candidates will have no sizable pool of new support to draw on. In contrast, as the party designed to appeal to voters as their second choice and win on the second ballot, the secondary party favors the strategy of the trial run. This means that the party seeks to get likely allies to agree to withdrawal for the second ballot on behalf of candidates who have demonstrated in the first round of elections the best chance for ultimate victory.[11] By encouraging competitive candidacies, this strategy naturally reduces the chances that the secondary party's candidates will amass an absolute majority on the first ballot. Conversely, it guarantees them a new source of support for the second ballot.

At the same time, the tensions inherent in alliances reinforce the positions of the variant prototypes, the dual electoral and marginal parties. Since the two alliance strategies are incompatible, tension is likely to be greatest between the primary party and its likely ally, the dual electoral party, which also aspires to win on the first ballot. Because the dual electoral party also seeks to appeal to voters as a first choice, it too finds an advantage in coming to terms with the primary party on a single candidate in many constituencies. Yet the dual electoral party may be less content with the strategy of the unique candidacy. For one thing, it requires hard bargaining between parties: the balancing of resources, of incumbency, of control of local offices in order to distribute candidacies. In such negotiations the advantage may well rest with the organization best constructed to win on the first ballot. For another, the dual electoral party also encompasses those who propose to win by expanding their support on the second ballot and who will therefore press for the strategy of the trial run. This pressure reinforces the dual electoral party's position as a distinctive type of organization.

Tension is more muted between the secondary party and its likely ally, the marginal party, which also takes most of its victories on the second ballot. This is because the trial run is the easier strategy to implement. Unlike the unique candidacy, the trial run can be implemented without much prior agreement or commitment between parties. While prior agreements and promises of mutual assistance can facilitate the alliance, they are not essential. Ultimately, the implementation of the alliance rests with the voters. The resolution of the difficult problem of whose candidate is to benefit from the alliance is turned over to the electorate. Nevertheless, the very nature of the alliance derives from the existence of two distinct types of organizations, one of which is adaptable, the other rigid. Hence, it eliminates neither tension between allies nor the pressure for the marginal party to maintain itself as a distinct organization.

The four prototypical parties should be stable over time regardless of the electoral outcomes in individual elections. The obvious reason is that officeholders whose techniques and strategies have been successful will dominate their organizations. Differences in electoral fortunes, on the other hand, will affect the two variant prototypes. While electoral prosperity will bolster their distinctive styles of winning and the organization traits that derive from these styles, electoral adversity will make the two variant parties look more like their prototypical allies. The reason is that in times of adversity, the variant parties will win in their strongholds where they have been most successful in negotiating the alliance strategies that derive from the two basic parties. Thus, in difficult times the dual electoral party's relatively fewer victories will come in districts where it is able to command a unique candidacy based on its ability to attract an absolute majority of the votes on the first ballot. Similarly, the marginal party's relatively few victories will come in districts where it can be expected to expand its support on the second ballot handily. This of course will also be true of the two basic prototypes. But there is an important difference. In good times and bad the two basic prototypes will rely mostly on their characteristic styles of winning. The variant parties will rely on the basic types' styles only in difficult times. Any improvement in variants' electoral fortunes will depend on candidates who win by departing from the styles of the two basic parties in the ways described. The variant parties will therefore retain the distinctive organizational traits we assigned them.

While the two-ballot system fosters four stable types of parties, it also allows parties to come and go. In the legislature this means the appearance of new groups that resemble one or the other of the prototypes, in part because they will draw some of their members from these sources. But it also means the disappearance of groups because their members choose to migrate to the prototype their previous group resembled. The two-ballot system may also encourage new political organizations to try their luck in electoral contests. At the same time, the greater inducement the system offers to the existent prototypical parties to continue to form alliances among themselves will give novice organizations little chance of electoral success.

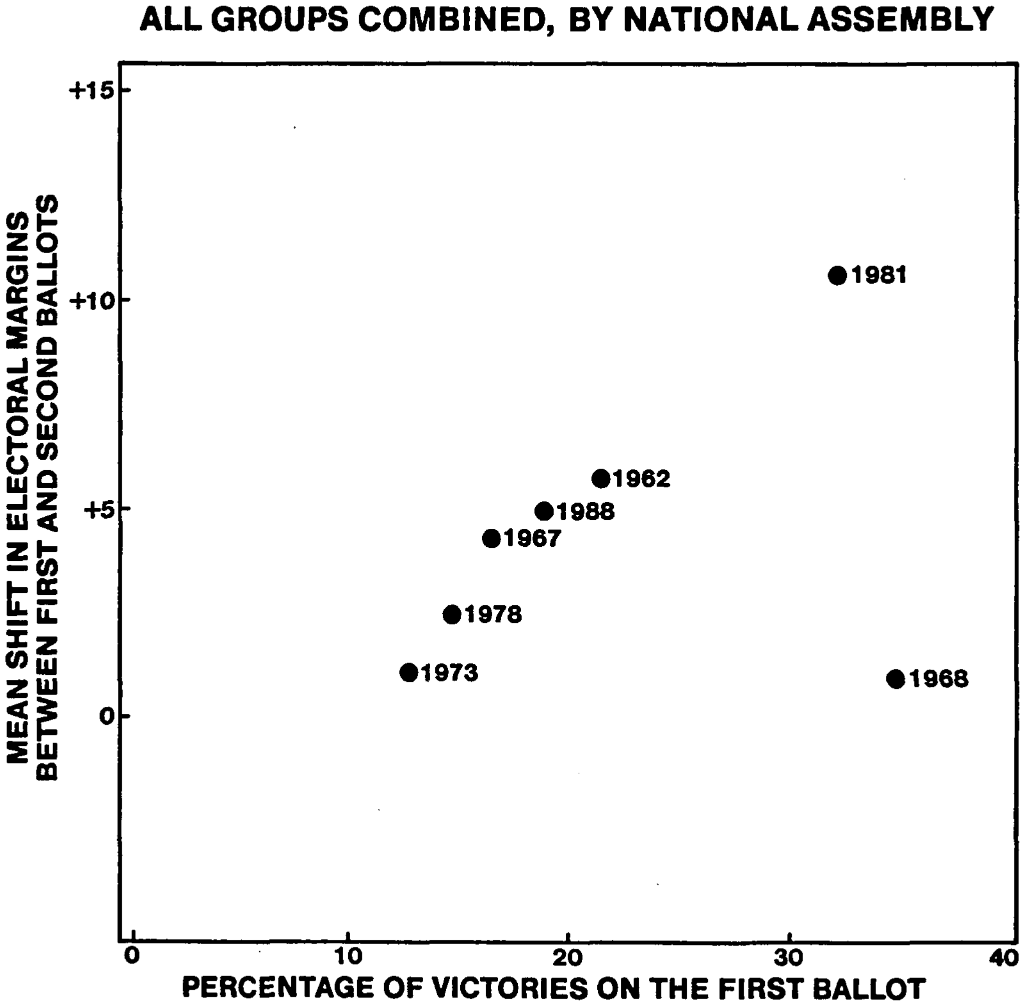
The Dual Ballot Typology and the Parties of the Fifth Republic

Our typology reveals the basis for the principal accomplishment of the Fifth Republic: the provision of reliable governing alternatives by means of four stable partisan organizations. In the 1960s, the period of Gaullist dominance, political observers called attention to the majoritarian government produced by the new Republic's party system. By the 1970s, as differences emerged in both governing and opposition coalitions—between Gaullists and the Independent Republicans-Union for French Democracy and between Socialists and the Communists—observers acknowledged the quadripartite character of governing alternatives (Dupoirier and Platone 1974; Duverger 1964; 1978, 154–75; Goguel 1967; Kesselman 1988; Le Gall 1977; Parodi 1977, 1978, 1979; Pierce 1980). Our typology reveals that both majoritarianism (in which one ally dominates the governing coalition) and quadripartism (in which coalitional allies are more equal) derive from the two-ballot arrangement introduced with the Republic's renovated structure of political opportunities.

Before applying our two measures of winning to the Republic's parties, we wished to test whether the relationship that produced our typology was built in to the two-ballot system of elections arithmetically. We looked first, therefore, at the overall results for the deputies elected to the seven assemblies between 1962 and 1988 on two ballots. We found that analysis of the overall electoral data, using our two measures of winning, produced results at variance with our theoretical assumptions. With the exception of 1968 there was a strong positive correlation between our two measures: the more first-ballot victories, the greater the positive shift in electoral margins on the second ballot (see Figure 2).

Only when we analyzed the results for each election by party did we find the negative relationships we expected. Figure 3 shows the breakdown of electoral results by partisan legislative groups for each of the seven assemblies.[12] In seven assemblies with strikingly different outcomes for the groups, the relationship between the two measures was always strongly negative. The positive relationship that emerged when electoral results were analyzed without regard for party, then, only

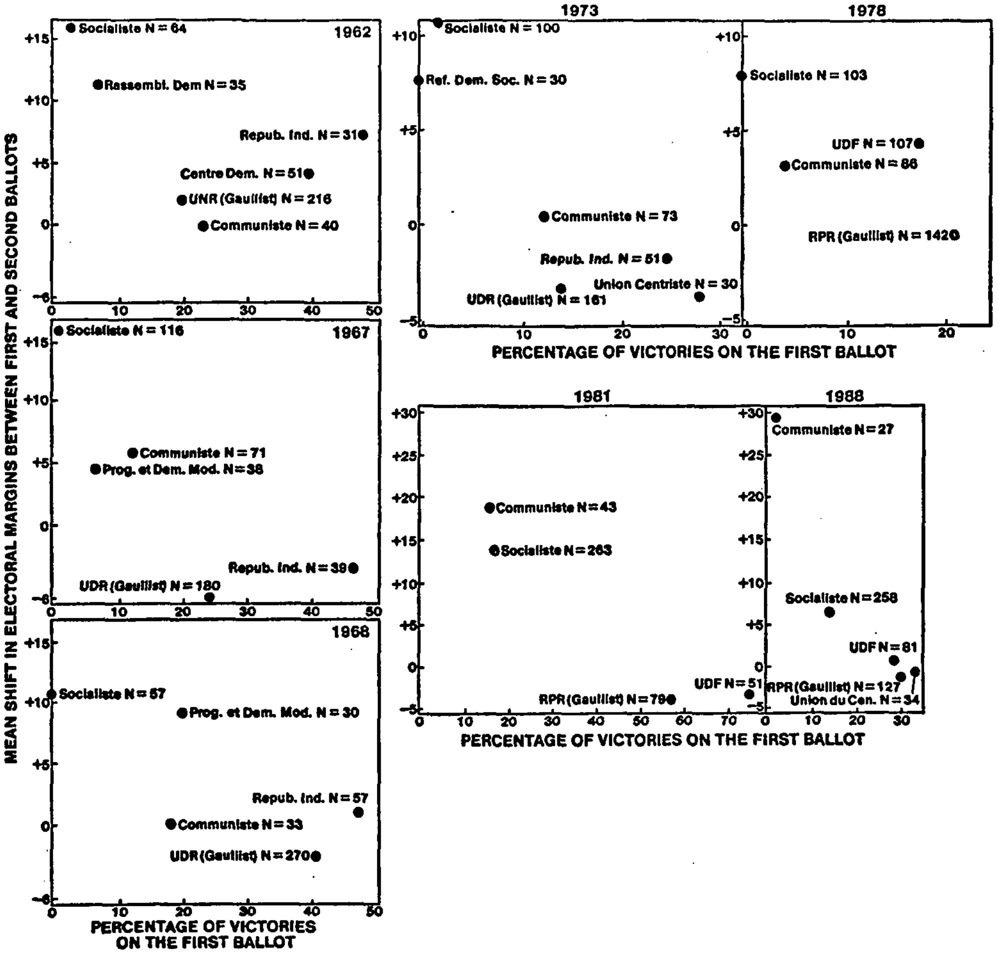
Figure 2. France: The Two-Ballot System of Elections to the National Assembly—All Groups Combined, by Assembly



Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: The total number of deputies was 488 in 1981 and 575 in 1988. The change in electoral rules in 1986 to proportional representation resulted in an increase in the number of deputies, which was retained with the return to the former electoral system. Correlations—*r* = .98 (excluding 1968); *r* = .39 (including 1968).

Figure 3. France: Political Groups and the Two-Ballot System of Elections to the National Assembly, 1962–1968



Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: Correlations—1962, $r = -.53$; 1967, $r = -.75$; 1968, $r = -.74$; 1973, $r = -.86$; 1978, $r = -.70$; 1981, $r = -.95$; 1988, $r = -.91$.

Table 1. Average Electoral Margins on the First Ballot of Deputies Having To Go to a Second Ballot, by Groups

Groups	1962	1967	1968	1973	1978	1981	1988
Communist	6.63 (31)	4.47 (63)	5.09 (27)	8.38 (65)	9.13 (82)	4.68 (36)	11.50 (26)
Socialist	.80 (62)	.67 (115)	-1.20 (57)	1.58 (99)	4.45 (103)	3.11 (217)	7.58 (222)
Gaullist	13.21 (173)	15.46 (138)	14.86 (161)	13.37 (140)	12.20 (112)	11.49 (33)	10.21 (89)
Independent Republicans	10.36 (16)	11.93 (20)	13.20 (30)	13.06 (39)	—	—	—
Union for French Democracy (UDF)	—	—	—	—	8.58 (87)	8.96 (13)	8.51 (58)
Centre Démocratique	7.47 (31)	—	—	—	—	—	—
Rassemblement Démocratique	4.05 (32)	—	—	—	—	—	—
Progrès et Démocratie Moderne	—	7.79 (36)	7.32 (24)	—	—	—	—
Union Centriste	—	—	—	10.68 (22)	—	—	—
Réformateurs Démocrates Sociaux	—	—	—	2.85 (30)	—	—	—
Union du Centre	—	—	—	—	—	—	5.50 (23)

Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: Number of deputies are in parentheses.

serves to confirm that the partisan differences we found were real and not a product of the arithmetic of the two-ballot system. Applying our two measures of winning to each of the groups elected in the seven assemblies, we did indeed find four groups that consistently resembled our four prototypical parties.

The Gaullists

Emerging clearly as the primary party was the Gaullist group in the national assembly. Only in 1973 did the group's proportion of first-ballot victories fall below 20% (see Figure 3). In every election after 1962 its proportion of first-ballot victories exceeded those of all other groups except its allies. Moreover, in every election except 1988, even Gaullist candidates who had to win on the second ballot amassed the highest average electoral margins on the first ballot (see Table 1). On the second ballot, however, these candidates won by compiling average margins of support that registered declines in every election except 1962. No other group's margins declined so consistently from one ballot to the other. This was a party whose support peaked on the first ballot, a party of first choice.

In the 1960s, as the fate of the new

Republic and its institutions remained unsure, the Gaullist, or primary, party dominated the regime. It did so by identifying the fortunes of the Republic with its own fortunes and identifying its own fortunes with Charles de Gaulle. This posture brought it strong support throughout the French electorate, as voting studies have demonstrated (Goguel 1963, 1971; Labrousse 1971; Lancelot and Lancelot 1971; Lancelot and Weill 1971).[13] As the elections of 1968 proved, numerous French voters had no firmer preference than the stability of the regime identified with Charles de Gaulle. (Converse and Pierce 1986, 417–43). Student and worker unrest brought the Gaullists the largest electoral victory that any party was to achieve through the eighties; 40% of these victories came on the first ballot.

In the 1970s and 1980s, however, once the Republic's institutions emerged as safe even in the event the opposition came to power, the status of the primary party became less secure. Gaullist representation in the national assembly declined, along with its margins of support between ballots (see Figure 3). No other group showed such large declines. Indeed, in 1981 the party suffered its worst electoral defeat, while emerging as a purer primary party than ever. In the 1981 elections almost 60% of the Gaullist group were elected on the first ballot—its record high; but between ballots the average margin of support for its winning candidates declined by 3.8%, the largest decline since 1967. As we had expected, in a period of political adversity the party had reverted even more to type. Even as the party's fortunes declined, it clung to its original organizational trait—domination by a strong personality seeking to personify a strong broad appeal. Now, however, the dominant personality was the controversial deputy mayor of Paris, Jacques Chirac, seeking to personify appeals incapable of achieving the consensus produced by the defense of the republic. These appeals ranged from the free market economy to Europe 92 and included an ambiguous flirtation with the anti-immigrant rhetoric of Le Pen's National Front.

The Socialists

Using our two measures of winning, we found the Socialist party to be the other basic type of organization, the secondary party. Even more than the Gaullists, the Socialists resembled the basic type of party: Figure 3 reveals strikingly the extent to which the Socialists were a secondary party. In the five elections from 1962 to 1978 only four of their candidates won on the first ballot. In two of these elections, 1968 and 1978, no Socialist candidate won on the first ballot. Even as the Socialist party was achieving its greatest electoral victory in 1981, it was electing only 17.5% of its deputies on the first ballot. In stark contrast, the Gaullists had elected 40% during their greatest victory in 1968. Moreover, except for the election of 1988, Socialist candidates who won on the second ballot compiled the lowest electoral margins on the first (see Table 1).

On the other hand the Socialists' second-ballot performance consistently outshone that of their competitors, except for the elections of 1981 and 1988. In these elections, as we shall see, the Communist party, faced with electoral adversity, moved toward the style of the secondary party. In these elections, however, the Socialists still continued to outperform the opposition by far on the second ballot, as they had in all previous elections. Even in 1962, as the Socialists were electing their second smallest number of deputies, their second-ballot margin of support improved by 16% compared with the Gaullists' 2.3%. In the electoral debacle of 1968 the Socialist party improved its second-ballot margin of support by 10.5%, while the Gaullists' margin declined by 2.1%. Of course, at this electoral low point, the Socialists were falling back on their basic technique of winning: in 1968 all their deputies won election on the second ballot.

The Socialists' performance certainly reflects Mitterrand's recognized transformation of their party into an acceptable governing party. Our analysis shows that Mitterrand was able to accomplish this by utilizing the party's appeal, visible since 1962, as the voters' preferred second choice. Under Mitterrand, the Socialists became increasingly successful at attracting Communist voters by assuring them of a continuing concern for their specific needs, while reassuring centrist voters that such concern would not threaten their interests.[14] Other electoral and voting studies have traced the Socialists' growing ability in the 1970s and 1980s to attract center as well as Communist votes for their candidates on the second ballot (Charlot 1973, 1981; Denis 1978; Frears and Parodi 1979; Goguel 1983b, 177–98; Jaffré 1981; Le Gall 1981; Martin 1981; Ysmal 1986). Our analysis underscores the party's growing success in targeting multiple groups of voters, in becoming the compromise choice of larger numbers of voters with different first ballot prefererences.

The Independent Republicans and Union for French Democracy

Apart from the two basic prototypes, our measures also revealed an organization resembling the dual electoral party: the Independent Republicans, together with its expanded version, the Union for French Democracy (UDF). In four out of the seven elections these groups had the highest percentage of deputies winning on the first ballot (see Figure 3). In the election of 1973, the Independent Republicans' percentage of first-ballot victories was exceeded only by the Union Centriste, which disappeared in 1978, its personnel mostly migrating to the UDF, as the theory led us to expect (see Table 2). In 1988 the highest percentage of first-ballot victories was achieved by the new Union du Centre, much of whose personnel came from the UDF, as we were also led to expect (see Table 3). Moreover, in every election except 1988 Independent Republican or UDF candidates who won on the second ballot were second only to the Gaullists in the size of the average electoral margins they compiled on the first ballot (see Table 1). On the second ballot the group consistently showed greater improvement in its average margin of support than the Gaullists; in three out of the seven elections it showed greater improvement than the Communists as well. Faced with electoral adversity in the election of 1981, the UDF did move clearly in the direction of the primary model: 75% of its deputies won election on the first ballot, the highest percentage won by any group in the seven elections; on the second ballot the decline in its average margin of support was its highest, equalling that registered in the 1967 elections by the Independent Republicans.

All the same the Independent Republicans–UDF were our prototypical dual electoral party. The small Independent Republican party was able not only to resist absorption by the Gaullists but also to expand its organization by joining with even smaller officeholders' fiefs to form the UDF as a confederation. Our analysis reveals a firm basis for the Independent Republicans' resistance to Gaullist pressures to transform a majority coalition into a majority party—a different and potentially more effective mode of winning. Here was a partisan organization that appeared to be capable of appealing to some voters as a first, to others as a second, choice. The potential was clearly present in the 1960s when in all three elections the smaller Independent Republican group always ran better than the Gaullists on both measures. With the organization of the UDF in 1978 the Gaullists found

Table 2. Ephemeral Groups: Migration of Their Personnel in the Succeeding Legislature

	Number in Next Legislature								
Groups	Socialist	Prog. et Dém. Mod.	Independent Republicans	UDF	Union Centriste	Ref. Dém. Soc.	Gaullist	No Group	Total
1962									
Rassemblement Démocratique (35)	22 + 1[a]	3	1[a]		—	—		1	28
Centre Démocratique (51)	—	18 + 1[a]	1		—	—	5 + 1[a]	1	27
1968									
Progrès et Démocratie Moderne (30)	—	—	1		12	9 + 2[a]		1	25
1973									
Independent Republicans (51)	—	—		24		—	1	1	26
Union Centriste (30)	—	—		9 + 3[a]		—	1[a]	1	14
Réformateurs Démocrates Sociaux (30)	1[a]	—		13 + 2[a]		—	—	1	17

Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: Number of deputies in the group are in parentheses.

[a] *Apparentés* (allied with the group).

their ally substantially increased in numbers; the improvement in UDF strength relative to the Gaullists was maintained in the 1980s (see Table 4). Examining the relationship between Gaullists and Independent Republicans, some electoral studies highlighted the similarity in their electoral and geographic support, implying a weak base for the smaller group's continued viability.[15] After the 1978 election other studies examined the UDF's ability, like the Socialists', to attract votes on the second ballot beyond their expected base of support (Charlot 1980; Frears and Parodi 1979, 83–97; Jaffré 1980; Laurens 1988; Parodi 1978, 26, 29). Our analysis confirms this advantage over the Gaullists while demonstrating that it was observable early on in the Republic.

The Communists

Finally, our two measures of winning revealed the Communist legislative group as the one most closely resembling the marginal party. After 1962 the Communist group always lagged far behind the Gaullists and the Independent Republicans–UDF in first-ballot victories (see Figure 3). Even the Communist candidates who won on the first ballot usually piled up smaller average margins than Gaullists and Independent Republicans (see Table 5). Until 1981 the Socialists always outshone the Communists on the second

Table 3. Source Groups of Ephemeral and Most Recent Groups' Personnel

	National Assemblies						
Groups and Source Groups	1962	1967	1968	1973	1978	1981	1986
Progrès et Démocratie Moderne, 1967 (38)							
Centre Démocratique	17 + 1[a]	—	—	—	—	—	—
Rassemblement Démocratique	3 + 2[a]	—	—	—	—	—	—
No group	2	—	—	—	—	—	—
Réformateurs Démocrates Sociaux, 1973 (30)							
Socialiste	—	2	2	—	—	—	—
Rassemblement Démocratique	4	—	—	—	—	—	—
Centre Démocratique	4	—	—	—	—	—	—
Progrès et Démocratie Moderne	—	8	—	—	—	—	—
Républicains Indépendants	—	1	—	—	—	—	—
Union Centriste, 1973 (30)							
Socialiste	—	1	—	—	—	—	—
Rassemblement Démocratique	2 + 1[a]	—	—	—	—	—	—
Centre Démocratique	5 + 2[a]	—	—	—	—	—	—
Progrès et Démocratie Moderne	—	11 + 1[a]	12 + 1[a]	—	—	—	—
Gaulliste	1	2	4	—	—	—	—
No group	—	1	—	3	—	—	—
Union du Centre, 1988 (34)							
Progrès et Démocratie Moderne	—	1	1	—	—	—	—
Union for French Democracy	—	—	—	—	9	11	22
Union Centriste	—	—	—	3	—	—	—
Réformateurs Démocrates Sociaux	—	—	—	3	—	—	—
No group	—	—	1	—	1	1	—

Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: Number of deputies in the group are in parentheses.

[a] *Apparentés* (allied with the group).

ballot. Moreover in three of the five elections before 1981 the Independent Republicans–UDF also outperformed the Communists on the second ballot. Here was an organization that clearly had difficulty appealing to voters as a first or second choice. Our findings merely confirm the rigidity of the Communist party, its inability to adapt to the alterations made in the structures of political opportunities and of electoral competition and to broaden its electoral appeal.

Only in the elections of 1981 and 1988, because it was doing more poorly than in any legislative elections since 1932, did the Communist group emulate the style of the secondary party and improve its performance on the second ballot. Certainly, the Communist performance in the election of 1988 corroborates our hypothesis that in times of adversity the variants would emulate the basic prototypes because of the need to fall back on their strongholds. In this election the Communists won almost entirely with candidates who demonstrated an impressive

Table 4. Relationship in Size between Principal Allies in the National Assemblies of the Fifth Republic

		Gaullist Ratio to		
National Assembly	Socialist-to-Communist Ratio	Independent Republicans	UDF	All Center Groups
1962	1.6	7.0	—	2.6
1967	1.6	4.6	—	2.3
1968	1.7	4.7	—	3.1
1973	1.4	2.0	—	1.5
1978	1.2	—	1.3	1.3
1981	6.1	—	1.6	1.6
1988	9.6	—	1.6	1.1

Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Table 5. Average Electoral Margins for Deputies Who Won on the First Ballot, by Groups

	Elections						
Groups	1962	1967	1968	1973	1978	1981	1988
Communist	33.4 (5)	28.9 (8)	23.2 (6)	33.7 (8)	33.8 (4)	27.8 (7)	25.9 (1)
Socialist	34.9 (2)	22.5 (1)	— (0)	22.7 (1)	— (0)	22.8 (46)	25.2 (36)
Gaullist	36.2 (43)	32.0 (42)	36.2 (109)	36.9 (21)	31.8 (30)	25.8 (46)	29.2 (38)
Independent Republicans	45.8 (15)	35.7 (19)	43.0 (27)	37.9 (12)	—	—	—
Union for French Democracy	—	—	—	—	34.8 (20)	25.9 (38)	26.5 (23)
Centre Démocratique	36.3 (20)	—	—	—	—	—	—
Rassemblement Démocratique	30.6 (3)	—	—	—	—	—	—
Progrès et Démocratie Moderne	—	26.7 (2)	32.6 (6)	—	—	—	—
Union Centriste	—	—	—	27.2 (8)	—	—	—
Réformateurs Démocrates Sociaux	—	—	—	— (0)	—	—	—
Union du Centre	—	—	—	—	—	—	31.7 (11)

Sources: Electoral data and group membership, 1962–1981 from Ministère de l'intérieur, *Les élections législatives de 1962, 1967, 1968, 1973, 1978, 1981* (Paris: Imprimerie Nationale); electoral data and group membership, 1988 from Le Monde, *Les élections législatives, 5 juin, 12 juin 1988* (Paris: Le Monde, Dossiers et Documents), and *Le Monde* 25 June 1988, p. 8.

Note: Number of deputies are in parentheses.

ability to improve their support on the second ballot. In 1988 the Communists registered the greatest improvement in the margin of support between ballots of any group in the seven elections. They did so by fielding a record number of candidates who ran on the second ballot without opposition: of the twenty candidates who ran without opposition in the 1988 election, nine were Socialists, while eleven were Communists. Coincidentally, the Communist candidates demonstrating unusual prowess on the second ballot were predominantly elected officeholders. Over half the Communist deputies elected in 1988 had served two or more terms in the assembly. Of the six new deputies, five were local officeholders elected in districts previously held by the Communists. Unfortunately for the Communists, officeholders capable of expanding their support on the second ballot were in short supply. In the 1988 election the Communists for the first time failed to win enough seats to form their own group in the national assembly. Only through the indulgence of the Socialists, who supported a rule change lowering the thirty-member minimum requirement for a group, were the twenty-seven Communist deputies elected to the 1988 assembly able to form their own group.

In analyzing the balance of power within the Communist-Socialist alliance of the Fifth Republic, electoral studies have traced the shift in dominance from Communists to Socialists to the 1970s. At that time Socialist candidates began to outpoll Communist candidates on the first ballot in elections. Voting and electoral studies accredited the shift in power to the Socialists' ability to attract a broader spectrum of support than the Communists. They found that voters were consistently more likely to support a candidate of the Left if the candidate were a Socialist (Baudoin 1981; Bon and Jaffré 1978; Charlot 1973; Frears and Parodi 1979, 89–97; Goguel 1983b, 59–92; Grunberg 1986; Grunberg and Schweisguth 1981, 152–55; Jaffré 1980; Jaffré and Ranger 1974; Lavau and Mossuz-Lavau 1980; Le Gall 1978; Parodi 1978; Platone and Ranger 1986; Ranger 1986).

Our analysis reveals that the Socialists' favored position was evident from the elections of the 1960s. In every election from 1962 on, the Socialists always elected noticeably more deputies than the Communists. From the elections of 1962 on, the Socialists' dominance of the second ballot was never in doubt. In the four elections between 1962 and 1973 the Socialists improved their electoral margins on the second ballot, as compared with the Communists, by at least 10 percentage points. Even in the election of 1978, when difficulties between the two parties undoubtedly cut into the improvement in the Socialists' second-ballot margins, they did better than the Communists by four percentage points. The Communists' larger second-ballot margins after 1978 came only as the ratio of Socialist to Communist strength in the national assembly increased dramatically. In the elections before 1981 the ratio of Socialist deputies to Communist deputies remained relatively stable (see Table 4). In 1981 the ratio rose to 6.1, in 1988 to 9.6. Our analysis, then, clearly relates the Communists' difficulties in the Fifth Republic to their emergence as the organization most closely approaching the marginal party. Only in times of adversity did the winning style of the secondary party become conspicuous.

Alliances between the Legislative Groups

We hypothesized that specialized parties devise strategies of alliance that reinforce their special positions. As our discussions of the Fifth Republic's legislative groups imply, this was the case. As the Republic's primary party, the Gaullists from their initial dominant position had sought conquest of, rather than cooperation with, their allies. Faced with determined resistance, they had used their considerable superiority of resources to impose upon the Independent Republicans the strategy of the unique candidacy. As the dual electoral party, the Independent Republicans were ambivalent toward but accepting of this strategy, given their junior status (Charlot 1971; 1973, 139–40; Goguel 1983a, 500–503).

In 1978, however, strengthened as a dual electoral party, the Independent Republicans, reincarnated as the UDF, came closest to practicing the strategy of the trial run. Even as tensions between the two allies reached their highest point, the UDF, bolstered by its newly confederated forces, as well as its control of the presidency, contested a record number of districts with the Gaullists, allowing the electorate to decide between them in contests that went to a second ballot (Charlot 1980, 81–87; Colliard 1979; Frears and Parodi 1979, 23–27, 32, 38–42). The result was an election that most strongly confirmed the UDF's status as the dual electoral party while bringing it closest to the Gaullist group in size. Faced with the prospect of defeat in the subsequent two-ballot elections, the UDF did revert to practicing the strategy of unique candidacy with the Gaullists (Charlot 1988, 62–69; Le Monde 1988, 36–37; Ysmal 1986, 177–78, 194–95). In these elections, the UDF also emulated more closely the primary party's style.

As we hypothesized too, the Socialists as the secondary party consistently engaged in trial runs with the Communists, the marginal party. For all seven elections the Socialists were able to implement this strategy with a more or less willing Communist party. It is true that during the 1970s, as it became increasingly clear the alliance was working to the Socialists' advantage, the Communists became increasingly uncooperative. For the election of 1978 they held aloof from any formal agreement until the eve of the second ballot for the first time since 1962. In this election Socialist progress was in any event affected by the explosion of candidates from minor parties, who in some instances deprived the Socialists of their favored status within the alliance on the first ballot (Boy and Dupoirier 1986; Frears and Parodi 1979, 18–23, 42–44, 73–75, 85–97; Parodi 1978).[16] In the two subsequent elections provoked by the Socialist president, however, the Communists had no choice but to accept without objection the strategy of the trial run. In these elections the strategy worked to the overwhelming advantage of the Socialists. But it also helped salvage the Communist legislative organization, which in adverse times had fallen back on the style of the secondary party.

As we hypothesized, the encouragement the two-ballot system gave to new electoral organizations did not disrupt the stability of the party system. Thus, the explosion of minor party candidacies in 1978, at most affected the distribution of power among the four major, stable groups rather than bringing new parties into the assembly. Given the increase in first-ballot contests between Gaullists and the UDF, the explosion of candidates affected the distribution of wins between these allies, as well as between the Socialists and Communists. Only the proportional representation system used for the 1986 election to the national assembly brought Le Pen's National Front into parliament. While the return to the single-member-district, two-ballot system in 1988 destroyed Le Pen's organization in the assembly, it allowed the Communist organization to survive. The legislative election of 1988 then left the multiparty system composed of our four prototypical parties intact.

Legislative Groups and the Presidency

We should note that the organizational and coalitional arrangements originating in the legislature have had their impact on partisan organization for the presidency. Also conducted in two rounds, the presidential elections have produced, for the most part, candidates from the four major legislative groups. The Communists, regretting their decision to sit out the presidential elections in 1965 and 1974, fielded candidates in 1981 and 1988. Neither the Gaullists nor their allies have been willing to pass up a try for the presidency. Certainly, the insistence of both on running candidates contributed to the close election of the Independent Republican Giscard in 1974, his defeat in 1981, and the defeat of the Gaullist Jacques Chirac in 1988. Rather than inspiring unity, defeat has exacerbated the differences between allies, thereby strengthening the multiparty system whose origins derive from the legislature.

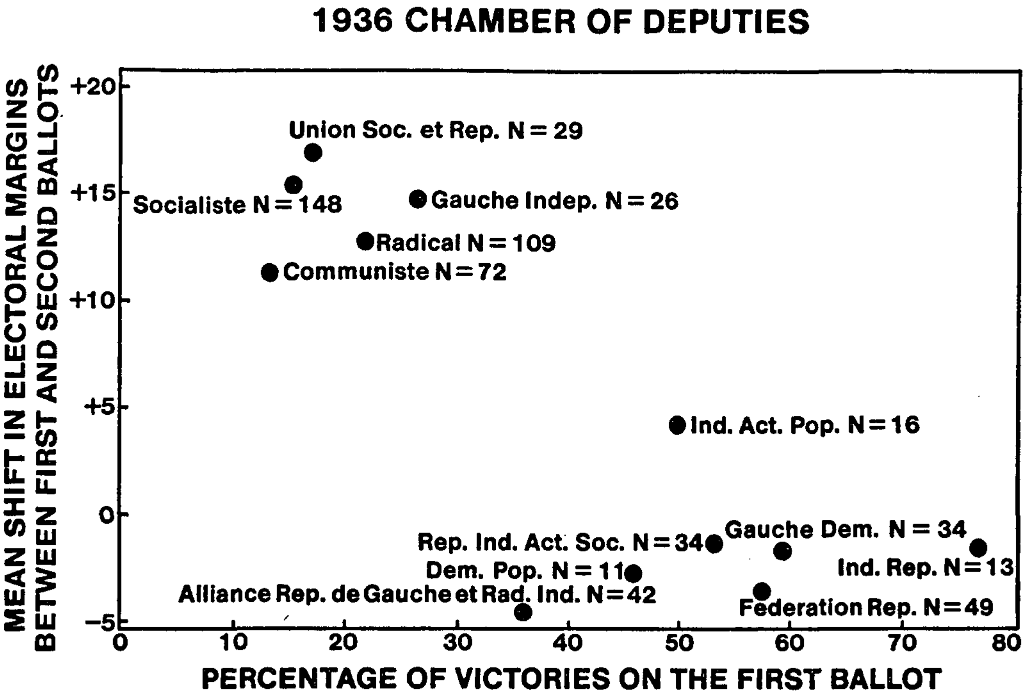
The Dual-Ballot Typology and the Parties of the Third Republic: A Retrospective

Since the *Third* Republic also used the single-member-district, two-ballot system for elections to parliament, we speculated that in the Third Republic, as in the Fifth, parties specialized in ways of winning elections. Since the four types of parties we discovered in the Fifth Republic could all trace their political ancestry in some fashion to the Third, we wondered if their specializations had originated there. To test our hypotheses we applied our two measures of winning to the groups formed by deputies elected in 1936 to the final legislature of the Republic. We were thus able to glimpse the parties of the Third Republic in their final stage of organization. The first thing that strikes us in Figure 4 is the large number of groups compared with the legislatures of the Fifth Republic.[17] A more careful examination shows the groups clustering acording to our two basic forms of organization: the primary and secondary prototypes.

Emerging as secondary parties were the Radicals, Socialists, and Communists.[18] The placement for the Radicals is the expected one. Throughout the Third Republic the party reigned as the pivotal political organization. From its founding in 1901 the Radical party had laid claim to the reformist position, translating its message—neither reaction nor revolution—into an effective electoral strategy for garnering second-ballot support (M. Schlesinger 1974). In 1936, however, the Socialists demonstrated that they had not only adopted the Radical style of winning, but were capable of using it more effectively. In 1936 the Socialists for the first time surpassed the Radicals in the number of deputies elected; for the first time they became the single largest group in the chamber (M. Schlesinger 1978). In the 1962 legislature both Radicals and Socialists reemerged as secondary parties. But the Radicals' small group, the Rassemblement Démocratique, was gone in the next legislature, its personnel having merged for the most part with the Socialists. The remaining Radicals merged with groups, bordering on dual as well as secondary parties, which ultimately joined the UDF (see Table 2). The Socialists' durability as a secondary party outlasted not only the Radicals but also the Communists. In 1936 the Communists' placement as a secondary party brought them their greatest electoral success in the Third Republic: they elected a record number of deputies, reversing the humiliating defeat of 1932 when their representation in the chamber had sunk to its all-time low. Unlike the Socialists, the Communists were unable to reemerge as a secondary party in the Fifth Republic, as we have seen.

In 1936, as in the Fifth Republic, the secondary parties employed the alliance strategy we predicated from our theoretical assumptions. Running on their own on

Figure 4. Political Groups and the Two-Ballot System of Elections to the Chamber of Deputies, 1936



Sources: Electoral data and group membership, 1936: Georges Lachapelle, *Élections législatives, 26 avril et 3 mai 1936, résultats officiel.* Paris: *Le temps; Journal officiel,* Chambre débats, 2d sess., 19 June 1936, pp. 1522–24.

the first ballot, they agreed on the second ballot, for the most part, on a single coalitional candidate. There is no question that the electoral success of the Communists and Socialists in 1936 resulted from the formation of the Popular Front alliance that made possible the implementation of this strategy. The Popular Front, formed two years before the elections as an antifascist coalition among Communists, Socialists, and Radicals, allowed Socialists and Communists time to mute their sharper ideological images and to reach out to a broader spectrum of voters than had been their wont (M. Schlesinger 1978). Thus, in the elections they were able to pick up support on the second ballot not only from each other but also from their more moderate partner, the Radical party.

Almost all the remaining groups cluster as primary parties. While the lines to the parties of the Fifth Republic are less clear, most can qualify as ancestors of the Gaullists or the UDF, having participated in governing coalitions that excluded Socialists, Communists, and sometimes Radicals. Looking most like a precursor of the Gaullist organization was the Fédération Républicaine. Founded in 1903 to combat the recently formed Radical party, the Fédération emerged over the course of the Third Republic as the most sharply defined conservative organization, bearing a strong Catholic and nationalist message. Moreover, during the interwar years it became identified with a single strong personality, the deputy Louis Marin. It is not surprising, then, to find this organization, like the subsequent Gaullist organization, a first-choice party. Equally long lived though less sharply defined were the two groups the Gauche Démocratique et Radicale Indépendante and the Alliance des Républicains de Gauche et des Radicals Indépendants. In this respect they were the forerunners of the Independent Republicans and the UDF. Their status as primary parties in 1936 undoubtedly reflects their fallback position in a polarized election where they were not favored.[19]

Indeed, in the elections of 1936 primary parties also followed the alliance strategy we posited for them. This finding is even more impressive, given the number of primary parties that participated. All the same, Georges Dupeux, in his excellent monograph on the 1936 elections, found that the opponents of the Popular Front had agreed on unity or quasi-unity candidates in more than half of the contested districts. Commenting on this strategy, he observed, "This tactic prevented the voters from expressing their preference for such and such political nuance; it allowed them only to show their hostility for candidates of the Popular Front" (Dupeux 1959, 125). These findings on the alliances of 1936 and the parties that formed them lend additional support to our assumptions about the dual ballot's effect on party organization in France.

Conclusions

Our findings about the parties of the Fifth Republic lend strong support to the central assumption of the office-seeking theory of parties in democracies: party organization reflects, above all, the response to the structures of political opportunities and of electoral competition. By introducing the popular election of a president endowed with considerable powers over the legislature, the Fifth Republic forced the restructuring of parties as national organizations capable of concerted, disciplined action in government. At the same time, the alterations in the structure of political opportunities did not diminish the key role played in party organization by the legislative group. Moreover, the structure of electoral competition that the Fifth Republic adopted, dual-ballot elections in single-member districts, ensured that the legislative group would be the core party unit in a multiparty system.

The multiparty system was, however, distinctive, consisting of parties tailored to the two-ballot mode of winning. Students of electoral systems have for some time recognized the effects of voting arrangements on the number of effective parties in a democracy. Our study reveals the impact of these arrangements on the parties themselves.

As we have shown, the parties of the Fifth Republic specialized in their approach to the dual-ballot election. Thus, the Gaullists emerged as a primary party, maximizing their strength on the first ballot largely because of their ability to create an organization with a strong appeal (defense of the regime) identified with a popular personality (Charles de Gaulle). They were also able to use their organizational strengths to implement a distinctive strategy of alliance—the unique candidacy—which reinforced their position as a primary party. On the other hand, the allies with whom they practiced this strategy, the Independent Republicans–UDF, had their own organizational response to the two-ballot election, that of the dual electoral party. This created tensions within the alliance, while reinforcing the position of the Independent Republicans–UDF as a distinct political organization. At the same time, the Socialists emerged as a secondary party, maximizing their support on the second ballot, largely by creating an organization that appealed to multiple groups of voters as the acceptable second choice. They, too, implemented a distinctive strategy of alliance that worked to their advantage, the strategy of the trial run. Yet tensions also existed within this alliance because the Socialists' allies, the Communists, had their own organizational response to the two-ballot election, that of the marginal party. These tensions reinforced the Communists' independent position. We have also shown that in the *Third* Republic the same electoral rules produced similar organizations, to which the parties of the Fifth Republic could trace their ancestry. These findings are important because they allow us to see significant similarities and differences in French parties that would otherwise be obscured.

Our findings also have broader implications. The French dual-ballot election exposes a problem inherent in all democratic elections, the problem of voters' preferences. In societies of any size or complexity we should not expect any single candidate or party to be the first choice of large numbers of voters. This basic insight tends to be overlooked, however, because most electoral systems give voters only a single option, the option to vote once for a single candidate in a plurality election or the option to vote for a list of candidates in an election determined by proportional rules. In these situations the problem of voters' preferences must be resolved within the party and among its candidates. The French dual-ballot election, however, while not offering voters the opportunity to rank all their preferences, does invite parties to approach voters so as to allow them to express either their first or compromise choices. Thus, analyzing the French structure of electoral competition for its impact on party organization can shed light on the tensions that exist within and between political parties in all democracies.

Notes

We thank Didier Maus, Jean-Luc Parodi, Roy Pierce, Marie-France Toinet, and Colette Ysmal for helpful discussions on our approach to the parties of the Fifth Republic.

1. On the relationship between president and parliament see "Le parlement français sous trois présidents 1958–1980," 1981, and "La constitution de la Cinquième République," 1984. See also Andrews 1978, Baumgartner 1987, Maus 1987, and Toinet 1988.

2. The problems of preference ordering and the "sincerity" of the vote are focal issues in positive political theory. Early discussions are Arrow 1951 and Black 1958. See also Riker and Ordeshook 1973, chap. 4.

3. For the effect of the single-member plurality election on the number of parties see E. E. Schattschneider's (1942, chap. 4) classic explanation and Duverger's (1951, 236–309) comparative analysis.

4. The Third Republic used the two-ballot system for elections to the chamber of deputies. For elections to the national assembly, the Fourth Republic used a list system, which distributed seats in some proportional fashion. Except for the legislative election of 1986, which used a form of proportional representation, the Fifth Republic has used the two-ballot system for legislative, presidential, and cantonal elections. For municipal elections a combination of the two-ballot system and a list, proportional representation system is used. A list, proportional representation system is used for elections to the European parliament and was also used in 1986 for the first popularly elected regional councils.

5. In the United States, primary nominating elections give voters additional options, thus greatly affecting U.S. party organization. In most primaries, however, voters, again, have only one option, since winners need only a plurality of the votes. Only runoff primaries offer voters a range of options similar to the range offered by the French two-ballot system.

6. Converse and Pierce (1986, chaps. 10–13) examine the behavior of the French electorate in the two ballots of the 1967 election.

7. In all of our calculations we have used the total votes cast (*votants*) in each constituency, rather than the more commonly used total of valid votes cast (*suffrages exprimés*). Since voters casting blank (*nul*) ballots can be expressing a preference for none of the candidates and since our theory rests on the importance of voters' preference ordering for parties, we decided to include their votes in the totals.

8. Since, as we shall see, the dual-ballot system encourages multiple parties, legislators who win on the first ballot by an absolute majority usually win by very comfortable margins. For the seven two-ballot elections held between 1962 and 1988, the

first-ballot winners' average electoral margin—that is, the difference between the winner's vote and that of the nearest rival—was 32.3%. In two-party terms that is equivalent to winning by over 65% of the vote.

9. The electoral margin is the difference in percentage of the total vote received by the winning candidate and the candidate receiving the next highest number of votes. On the first ballot, if the eventual winner did not come in first, the margin is negative, because it is calculated as the difference between the winning candidate's vote and that of the candidate who led on the first ballot. The figures we have used are the differences between the electoral margins on the second and first ballots, averaged for each group as a whole.

10. Contrary to the rule in the Third Republic, new candidates cannot enter for the second ballot and candidates can run on the second ballot only if they receive a certain percentage of the votes on the first ballot. The requirement was, for the election of 1962, 5% of the *suffrages exprimés*; for the elections of 1967, 1968, and 1973, 10% of the registered voters (*electeurs inscrits*); for the elections of 1978, 1981, and 1988, 12.5% of the *electeurs inscrits*. The law does provide that if only one candidate meets the requirement, the next highest vote getter can contest the second-ballot election; if no candidate qualifies, the two highest vote getters on the first ballot contest the election.

11. This type of alliance is often referred to as a "primary" to suggest a similarity to the U.S. nominating primary. A significant difference is that electorates in U.S. primaries are restrained or restricted.

12. In analyzing the groups, we include deputies from both metropolitan France and the overseas possessions. We did not include those allied to a group (*apparentés*), because we considered their organizational commitment weak. Nor did we include the small number of deputies listed in each legislature as "belonging to no group."

13. The Gaullists played up their identity with the new Republic with the labels they used for their candidates in the elections of the 1960s: in 1962, Union pour la Nouvelle République; in 1967, Comité d'Action pour la Ve République; in 1968, Union pour la Défense de la République.

14. Mildred Schlesinger, "Why Mitterrand Is Qualified To Make Power Sharing Work," *Christian Science Monitor*, 13 April 1986.

15. Studies of the elections held in the 1960s did not even distinguish between the support of the Independent Republicans and the Gaullists (Goguel 1963, 1971; 1983a, 500–503; Lancelot and Lancelot 1971; Lancelot and Weil 1971). Colliard (1972, 207–34) raises the difficulties of distinguishing between the Gaullists' and the Independent Republicans' electoral support in the sixties. Converse and Pierce (1986, 30, 32, 38, 114–15, 189, 218, 262–63, 266, 435–36) stress the similarities between the two in the sixties. Analyzing the results of the 1973 election, Charlot (1973, 81–144) found the Independent Republicans and the Gaullists sharing the same electoral fate. See also Hayward and Wright 1973.

16. In the 1978 election the Socialists also faced competition on the first ballot for the first time from the small Mouvement des Radicaux de Gauche (MRG). In the past the Socialists and the MRG had easily come to terms on candidates before the first ballot, thanks to the special relationship between the Socialists and this remnant of the Radicals that had carried over from the Fourth Republic.

17. In the Third Republic only 13 deputies were needed to form a group. Institutional rules also favored small groups and their pivotal bargaining power for positions in the executive (M. Schlesinger 1989).

18. The two other groups that emerged as secondary parties were organizations whose personnel always associated with the Radicals or Socialists. The Gauche Indépendante was an ephemeral organization that appeared only in 1936 and disappeared from the chamber of deputies by 1938. The Union Socialiste et Républicaine had a more continuous history, tracing its origins to the founding of the Socialist party in 1905, whose revolutionary rhetoric its members rejected. In 1936 the group received new recruits from those breaking with the Socialist party over its refusal to support Radical cabinets in the previous legislature.

19. The remaining primary parties, all newer, more ephemeral organizations, nevertheless had sharper images than these two groups. They ranged from the tiny Groupe Démocrate Populaire (a Catholic party) and the Groupe des Républicains Indépendants et Agraire Indépendant (an agrarian party) to the Groupe Indépendant d'Action Populaire (formed to defend the interests of Alsace-Lorraine)—all of which appear appropriately as primary parties. The tiny Groupe des Indépendants Républicains (to be distinguished from the Independent Republicans of the Fifth Republic) consisted of deputies brought together by the rule that required all deputies to form some type of group. Their first ballot strength supports their position as political powers in their own right (Anderson 1974; Irvine 1979).

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CANDIDATE EQUILIBRIUM AND THE BEHAVIORAL MODEL OF THE VOTE

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Most applications of spatial modeling to the problem of electoral competition are pessimistic regarding the prospects for candidate equilibrium in more than one policy dimension. Probabilistic models of the vote, however, increase the likelihood of equilibrium. We expand the probabilistic model to include measured nonissue variables, thereby representing the general multivariate model of behavioral research. For this model we offer a general candidate equilibrium solution and illustrate with some simulations based on 1988 National Election Study data. The more complicated one's model of voters' motivations, the greater appears to be the chance of locating a candidate equilibrium position in policy space.

The role of policy issues in electoral politics has been the focus of two very different research approaches. One approach, tracing back to Downs's (1957) *Economic Theory of Democracy* and before, is that of rational choice. Rational choice theorists focus on spatial models of candidate competition for votes from an issue-oriented electtorate. The second approach, tracing back to Campbell and his colleagues' (1960) *American Voter* and before, is that of political behavior research. Behaviorists emphasize the empirical examination of voting behavior, with a heavy reliance on survey research. These two rich research traditions have evolved quite independently of each other.

From their rational choice perspective, spatial modelers generally (but with important exceptions to be noted) assume that issues drive voting decisions in a deterministic fashion, with voters *always* voting for the candidate closest to their views. Although this assumption of an issue-oriented electorate is a simplification, it seemingly facilitates the theorizing about candidate issue strategy and election outcomes. The general quest is for a candidate policy equilibrium—a location in policy space to which knowledgeable candidates should gravitate if they want to win elections.

Researchers who study elections from a behavioral perspective tend to see more than policy issues motivating individual voters. In the behavioral literature, the presence of an issue-oriented electorate is far from a given. Indeed, a central controversy in the behavioral literature is whether issues matter much at all to an inattentive electorate that often seems more readily swayed by other variables such as party identification, candidate images, and retrospective evaluations of incumbent performance.

Most rational choice models ignore party identification and other nonissue variables that the behaviorists find interesting. Suppose, however, that it were

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possible somehow to incorporate the behaviorists' more cluttered but more complete multivariate model of voter motivations into the spatial modelers' sophisticated models of candidate behavior. This would seem to be a daunting task. After all, the raison d'être for the vast post-Downsian literature on spatial modeling is that the Downsian median voter result breaks down with the complication of multiple issue dimensions. Suppose we add the further complication of nonissue voter motivations in the form of both measurable variables (e.g., party identification) and unmeasurable variables (the error term). Is it possible to develop a useful theory of candidate issue strategies based on candidates who assume the behaviorist's multivariate model of voter motivations, which includes only a modest place for issue voting?

The surprising answer to this question is *yes*. In recent years, spatial modelers have gone beyond deterministic models of voting (where voters vote only on the basis of identified issues) to probabilistic models where individual voting decisions contain a random component (Coughlin 1986, 1990; Coughlin and Nitzan 1981; Enelow and Hinich 1982, 1984a, 1989; Hinich 1977, 1978). This random component can be modeled in many ways. One way is to model it exactly like the error term in the behaviorist's statistical model (e.g., probit) of the electoral decision (Enelow and Hinich 1982; 1984a, chap. 5). As we shall show, probabilistic models force some reinvention of the Downsian rules of candidate strategy.

The pathbreaking Enelow-Hinich model contains a random component much like the normally distributed error term of the behaviorist's statistical model. The theoretical result is a likely candidate equilibrium not at the median voter position but at the intersection of the electorate's (weighted) mean positions on the various policy dimensions. From an empirical standpoint, however, the addition of the random error component still leaves the Enelow-Hinich model underspecified. Still left out are the voter's party identification and evaluations of the candidates' personal characteristics, plus any other measurable nonissue variables that the behavioral researcher might include as a potential aspect of the voter's calculus.

We shall investigate the possibility of candidate equilibrium in policy space for the most general model of voter decision making. We assume the behavioral researcher's version of the voter's calculus, which can be modeled as a multivariate probit equation. We assume that voters decide on the basis of any number of measurable variables (such as party identification), plus some unmeasured influences (represented by a normally distributed error term and an intercept). Policy issues are included among the measured variables, in terms of candidate issue proximity on one or more policy issues. We ask whether this greater realism and complexity allows for candidate equilibrium in policy space and, if so, where this equilibrium is located.

A General Model of Candidate Equilibrium

Spatial modelers generally represent the voting decision as a deterministic function of issue proximity, with voters always voting for the candidate closest to their own issue position. More exactly, spatial modelers almost always conceptualize the *i*th voter's relative utility for one candidate versus the other as an exact function of the difference in squared Euclidean distance, or "quadratic loss":

$$U_i = \sum_{j=1}^{m} b_{ij}[(a_{ij} - y_j)^2 - (a_{ij} - x_j)^2], \qquad (1)$$

where U_i represents the voter's relative utility of Candidate X's election, compared to the election of Candidate Y. If U_i is greater than zero, the voter selects X; if less than zero, the voter selects Y. The right-hand side of equation 1 sums the quadratic loss differential for m issue dimensions. For the jth dimension, b_{ij} represents the voter's weight (salience) of the jth issue dimension, a_{ij} represents the voter's position on the jth dimension, and x_j and y_j represent the positions of Candidates X and Y, respectively, on the dimension.

The challenge is to determine the optimal issue positions for candidates. Optimal candidate positions can be portrayed as equilibria in a game among competing candidates. In a two-candidate election, a joint candidate equilibrium is a point in policy space where neither candidate would have an electoral incentive to leave. The best-known example of candidate equilibrium is Downs's median voter solution. For one dimension and deterministic voting, the equilibrium position is the position of the median voter. If both candidates are at the median voter position, neither has an incentive to leave, because the election would be won by the candidate who stays. If both candidates stay at equilibrium, the election result is a tie. Consider, however, more than one policy dimension. For deterministic models (that is, with voters deciding solely on the basis of policy issues), the Downsian "median voter" solution breaks down. Candidate equilibrium no longer exists except in the unlikely or contrived instance when a point in multidimensional space is a median in all directions. For every set of issue positions that a candidate might espouse, the opponent can choose another set that can defeat it. It is fair to say that the central focus of the spatial modeling literature since Downs's work has been to find ways of coping with the theoretical result of an electoral world in general disequilibrium. (For good reviews of the problem of disequilibrium in the context of two-candidate elections, see Enelow and Hinich 1984a, chap. 4; Mueller 1989, chap. 10; Ordeshook 1986, chap. 4; and Riker and Ordeshook 1973, chap. 11.) Recent research has focused on identifying the "uncovered set" in the center of issue space (Cox 1987; McKelvey 1986; McKelvey and Ordeshook 1976; Miller 1980), where candidates should prefer to locate. Still, within the preferred uncovered set, no single location dominates all others.

Probabilistic voting theory offers a potential escape from the inevitability of disequilibrium.[1] Equation 2 presents a translation of the Enelow-Hinich model of probabilistic voting:

$$\begin{aligned} U_i &= \sum_{j=1}^{m} b_{ij}[(a_{ij} - y_j)^2 - (a_{ij} - x_j)^2] + u_i \qquad (2) \\ &= Z_i + u_i, \end{aligned}$$

where u_i is a normally distributed disturbance term representing nonissue sources of the voter's relative utility for Candidates X and Y and Z_i summarizes the nonerror component on the right-hand side. As with the deterministic model, voter i votes for X if $U_i > 0$ and for Y if $U_i < 0$. But the random component u_i makes any voter's vote indeterminant from issue proximity alone. From equation 2 we can model $P(X)_i$—the probability that the ith voter will vote for X instead of Y:

$$P(X) = \frac{1}{\sqrt{2\pi}\sigma} \int_{-\infty}^{Z_i} \exp -\left(\frac{u^2}{2\sigma^2}\right) du, \quad (3)$$

where σ represents the standard deviation of the random disturbance term. In words, equation 3 states that the probability of voting for Candidate X equals the cumulative normal distribution at Z_i/σ. It is convenient to normalize this equation by setting σ equal to 1.0.

The Enelow-Hinich model produces equilibrium results quite different from those of the deterministic model. Whereas the deterministic model allows electoral equilibrium at the median voter position for one dimension, it results in general disequilibrium for multiple dimensions. Enelow and Hinich's model allows for possible equilibrium or disequilibrium for any number of issue dimensions. Profoundly, the Enelow-Hinich model rejects the hallowed median voter solution—even for a single issue dimension. For any number of dimensions, if an equilibrium point exists, this point is the intersection of the weighted means (not medians), where the dimensions are weighted by their salience. How the distribution of voters affects the prospects of equilibrium with the Enelow-Hinich model is only beginning to be understood (Enelow and Hinich 1989). But the number of issue dimensions is not a decisive factor. And Enelow and Hinich report one seeming perversity: the greater the random component of voter decision making (represented by the magnitude of the disturbance term), the greater the possibility of candidate equilibrium. In other words, candidates are more likely to have an optimal electoral strategy when individual voters are more heavily influenced by nonpolicy considerations.

Provocative though the Enelow-Hinich results are, the Enelow-Hinich model of the voter calculus is only marginally more realistic than the deterministic model. Behavioral researchers see the voting equation as both probabilistic and multivariate, front-loading the vote equation with a series of nonissue variables. With issue proximities measured in terms of quadratic loss, the behaviorist's version of the vote equation takes the general form:

$$\begin{aligned} U_i &= c_0 + \sum_k c_k p_{ik} + \sum_j b_{ij}[(a_{ij} - y_j)^2 \\ &\quad - (a_{ij} - x_j)^2] + u_i \qquad (4) \\ &= Z_i + u_i \\ &= K_i + J_i + u_i, \end{aligned}$$

where c_0 is a constant, the intercept of the regression model; $\Sigma c_k p_{ik}$ represents the linear effects (c_ks) of nonissue variables (p_{ik}s, e.g., party identification, candidate evaluations); and u_i again represents the normally distributed error term. The composite Z_i can be decomposed into J_i (the issue component) and K_i (the nonissue component).

We can also model $P(X)_i$ directly as a function of Z_i:

$$P(X)_i = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{Z_i} \exp -\left(\frac{u^2}{2}\right) du, \qquad (5)$$

where $\sigma_u = 1.0$. Equation 4 represents the *empiricist's regression model predicting* relative liking of the two candidates from a set of observable independent variables including the issue component, plus u_i—the normally distributed disturbance term that gives the voting decision its probabilistic flavor. Equation 5 is the companion probit model predicting the vote from the same variables. But we impose one difference. The empirical researcher would normally treat the candidate positions as fixed and the voter characteristics as the variables. Here we reverse these roles, assigning candidate positions the symbols x_j and y_j, which usually are reserved for variables. Given the behavioral model, we ask how the candidates can manipulate their x_js and y_js to maximize their electoral chances.

In principle, the answer is a joint candidate equilibrium point where (if both candidates locate there) both candidates would maximize their expected vote by staying rather than moving. Define $V(X)$ as the aggregate proportion of n voters who vote for Candidate X, or

$$V(X) = (1/n) \sum_{i=1}^{n} P(X)_i.$$

Candidate X's task is to find the vector of policy positions that maximize $V(X)$

against a rational opponent who tries to minimize $V(X)$. Assume that both candidates already stand at equilibrium on all issues except issue j. Candidate X can maximize the function $V(X)$ with respect to x_j when the partial derivative equals zero, or

$$\frac{\partial V(X)}{\partial x_j} = (1/n) \sum_{i=1}^{n} \frac{\partial P(X)_i}{\partial x_j} = 0$$

(and $\partial^2 V(X)_i / \partial dx_j^2$ is negative). The solution is the weighted mean

$$x^*_j = \frac{\Sigma w_{ij} a_{ij}}{\Sigma\, w_{ij}}\ ;$$

$$Q^* = (x^*_1, x^*_2, \ldots, x^*_m),$$

where x^*_j is the equilibrium point on the jth dimension, Q^* is the intersection of all x^*_j on the m dimensions, and

$$w_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp -\left(\frac{Z_i^2}{2}\right) \qquad (6)$$

$$= \frac{b_{ij}}{\sqrt{2\pi}} \exp - \frac{(J_i + K_i)^2}{2}\ .$$

With both candidates at equilibrium, $x_j = y_j$ for all m dimensions. The result is that the issue component (J_i) drops out of the exponential term and equation 6 simplifies to

$$w_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp - \left(\frac{K_i^2}{2}\right) \qquad (7)$$

$$= \frac{b_{ij}}{\sqrt{2\pi}} \exp - \frac{(c_0 + \Sigma c_k p_{ik})^2}{2}$$

which reduces to $b_{ij}/\sqrt{2\pi}$ (the Enelow–Hinich result) when $K_i = 0$. The proof is presented in the Appendix.

The weight w_{ij} represents not only b_i, the salience of issue i to the voter. The weight w_{ij} is the product of b_{ij} times the elasticity or marginality of the voter's decision based on measured nonissue variables. This conforms to common sense: the more uncommitted the voter's candidate choice, the more the candidates will take the voter's policy preferences into account. More exactly, the marginality component $(1/\sqrt{2\pi}) \exp - (K_i^2/2)$ represents the height of the normal curve at K_i, or $c_0 + \Sigma c_k p_{ik}$, where the c_k terms are the equivalent of probit coefficients. An alternative presentation is that the marginality term $(1/\sqrt{2\pi}) \exp - (K_i^2/2)$ represents the slope of the cumulative normal distribution at value K_i. A voter's marginality is maximal at $K_i = 0.0$ or when the probability $P(X)_i = .50$.

Mathematically, the weighted-means equilibrium point is only guaranteed to be a *local* equilibrium. This limitation means that if both candidates are at Q^*, we are sure only that neither would gain votes by moving *a short distance* from equilibrium. Whether Q^* is a *global* equilibrium becomes problematical. If Candidate Y is at Q^* when Q^* is only a local equilibrium, Candidate X's best defense would be some different position that represents a larger maximum, given Y at Q^*. Opponent Y could find the best location against X's new position, and so on; and (in principle) endless cycling could ensue. But recall that for probabilistic models the likelihood of a global equilibrium is proportional to size of the random disturbance term in comparison to the degree of issue voting (Enelow and Hinich 1989). Since voters supposedly engage in little issue voting, we should be encouraged about the possibility of global equilibria among actual electorates in the real world.

Hypothetical Examples

Some hypothetical examples may provide some intuition for our equilibrium results. Figures 1 and 2 show how the vote can vary probabilistically as a function of the voter's position and the candidates' positions on a single dimension:

$$P(X) = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{Z_i} \exp - \left(\frac{u^2}{2}\right) du,$$

where $Z_i = b[(y - a_i)^2 - (x - a_i)^2]$ for one issue dimension, with nonissue component J_i set to zero for all voters and b constant for all voters. As for all the voter equations that follow, the equation is normalized so that the variance of the disturbance term is assumed to equal 1.0. (This follows the analogous probit convention that the standard error of estimate equals 1.0.)

The slopes shown in Figures 1 and 2 take the form of *S*-shaped normal ogives. Figure 1 holds constant the candidate positions (Y at −1, X at 1) and shows the slope varying with the salience weight (b) set variously at .25, 1.00, and 4.00. The greater the salience of the issue dimension, the more issue voting. Figure 2 shows that the same variability can be obtained by holding constant the salience weight (b = 1.00) and varying the candidate distances. For Figure 2, candidates are set at −.25, .25; −1, 1; and −4, 4. The more distant the candidate positions from one another, the more issue voting. This theoretical result conforms with the common empirical finding that issues matter in proportion to the degree that the candidates disagree on the issues (Nie, Verba, and Petrocik 1976, chap. 18; Wright and Berkman 1986). Moving the candidates apart on the issues is the functional equivalent of creating a more issue-oriented electorate.

Equilibrium in One Dimension

We shall now consider the location (if any) of the candidate equilibrium for the three different b values shown in Figure 1. For this exercise, we invent hypothetical electorates, represented as three equally sized voter groups: A at −1.00, B at .40, and C at .60. With probabilistic voting, the local candidate equilibrium is at the mean, zero. (The unweighted mean is the equilibrium point because w_{ij} is the same for all n voters.)

For each of the three b values, Figures 3–5 show the vote for each group, plus the aggregate vote for all three groups, as a function of candidate positioning. We fix Candidate Y at (local) equilibrium point 0.0. Then we move Candidate X across the range of issue space and observe the vote for X among groups A, B, and C. Our central concern is the *aggregate* vote of the three groups as a function of X's location. Is there any loca-

Figure 1. Probabilistic Voting in One Dimension: Varied Salience, Constant Candidate Positions

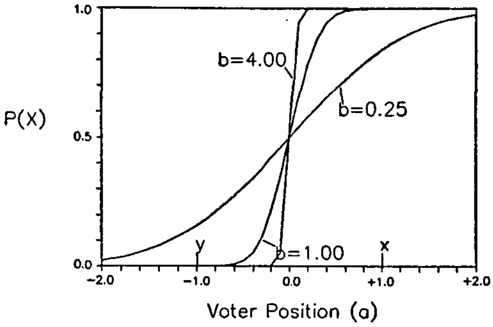
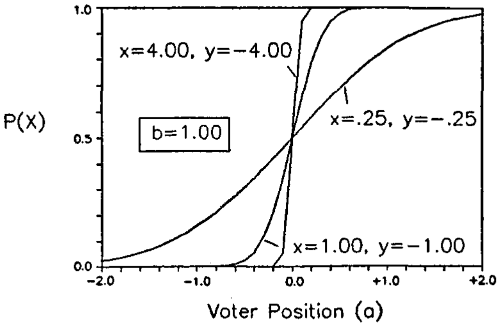


Figure 2. Probabilistic Voting in One Dimension: Varied Candidate Positions, Constant Salience



tion at which X obtains more votes than at local equilibrium 0.0?

The first thing one notices is that the more "extreme" groups (A and C) appear more responsive to candidate movement than does the middle group (B). This is not because the more extreme groups weigh issues any more heavily. Rather, given the quadratic loss model, voters respond more to candidate differences when the candidates are farther from the voter. This responsiveness of extremist voters to candidate differences at the center helps drive the system to equilibrium.

For Figure 3, the voters' issue salience (*b*) is set at the "mild" position of .25 for all voters. With this model, the mean 0.0 is clearly a global equilibrium. When Y is at equilibrium point 0.0, Candidate X's maximum expected vote is also at 0.0. For any other location, including the median (.40), X gets fewer votes than at the mean. If X moves toward the median, the slight gains among B- and C-type voters on the moderate right are outweighed by the greater losses among extremist A voters on the left. If X moves in the opposite direction, toward A, losses among B and C voters outweigh gains among A voters.

Next observe Figure 4, with its "medium" (*b* = 1.00) degree of issue voting. Now if Candidate X approaches the position of either A or C voters, that candidate obtains a much greater share of the group's voters. Candidate X's aggregate vote is once again maximized at the mean 0.0.

For Figure 5, the voters' issue saliences are quadrupled again (*b* = 4.0) to produce a "high" and almost deterministic degree of issue voting. Now, the mean 0.0 is only a local equilibrium, as Candidate X can defeat Y by moving more than a short distance to the right of 0.0. With Y at Q^* (0.0), X's vote maximum is slightly to the right of the median voter position .40. But this new position (*M*) is not an equilibrium position for X, because Y can

Figure 3. Vote for Candidate *X* by *X*'s Location, Candidate *Y* at Equilibrium (*b* = .25)

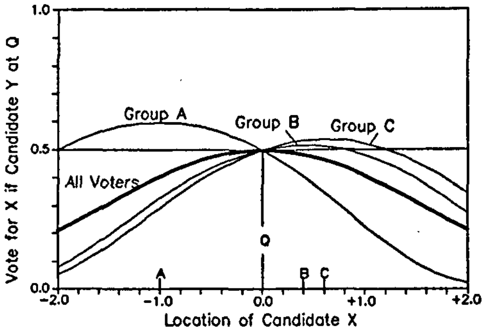


Figure 4. Vote for Candidate *X* by *X*'s Location, Candidate *Y* at Equilibrium (*b* = 1.00)

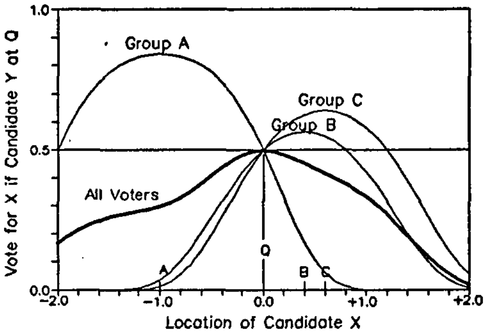
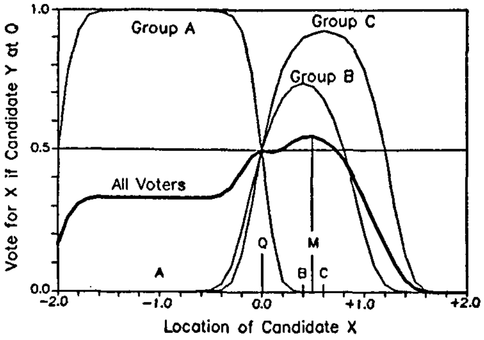


Figure 5. Vote for Candidate *X* by *X*'s Location, Candidate *Y* at Equilibrium (*b* = 4.00)



counter by taking a position to its left but to the right of the mean. Potentially, the candidates could cycle endlessly in the range of the median. As *b* becomes very large, deterministic voting is approached, and the range of cycling around the median becomes infinitesimally small. Downs's median result is then achieved, but only for the special condition of purely deterministic voting.

Because the median voter result no longer holds, and because there may not even be a candidate equilibrium with one issue dimension, the probabilistic model of voter decision making may seem to be a source of instability. However, for global equilibrium not to hold, a large degree of issue voting, such as that shown in Figure 5, is required. More specifically, the error variance must be small relative to the range of opinion. For example, equilibrium could be restored even to Figure 5 by the simple contrivance of adding one extra voter group in the vicinity of the current mean. Moreover, the probabilistic model offers one important source of stability—an improved potential for equilibrium when the number of issue dimensions is greater than one.

Equilibrium in Two Dimensions

For the usual deterministic spatial model, the equilibrium solution of the median voter position generally breaks down for two or more dimensions. The reason is that there almost never exists a location that is a median position in all directions. For probabilistic voting, however, the (local) equilibrium position of the intersection of the (weighted) means always exists for all directions. But also, as for one single dimension, this local equilibrium may or may not be a global equilibrium.

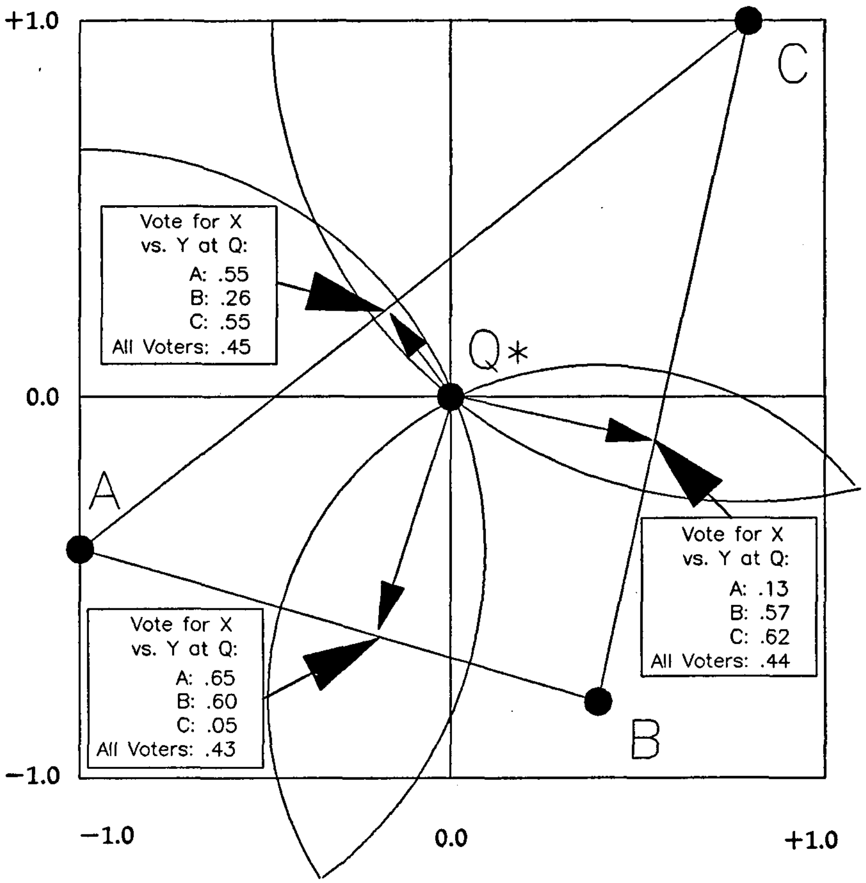
Figure 6 recreates the hypothetical election of Figure 4, but now with a second issue dimension. Our three voter groups (A, B, and C) are spaced on the horizontal axis, as before. But for Figure 6, they also vary on the vertical axis. With deterministic voting the configuration of Figure 6 would yield no unique equilibrium, since (as usually for multiple dimensions), the configuration contains no median in all directions. With probabilistic voting, the equilibrium point is the intersection of means (Q^* in Figure 6). If voting were deterministic, a candidate at Q^* would lose to an opponent who moved from Q^* in the direction of two of the three voter groups. But given the probabilistic equation for Figure 6, such movement attracts fewer voters in the groups moved toward than it loses from the group the candidate moves away from. Figure 6 shows the details.

Adding Party Identification

The examples shown in Figures 3–6 illustrate the Enelow-Hinich model of probabilistic voting based on issue proximity plus an error term. Next, we complicate the voter model by introducing a measured nonissue variable, party identification. The addition of party identification (or any measured nonissue variable) would have no effect on the equilibrium point if it is distributed orthogonally to (statistically independently of) voters' spatial alignments. But suppose that our three-group electorate has built up a culture of voting habits (party identification) that correlates with the issue dimension in the same way that actual party identification in the United States correlates with various measures of policy liberalism-conservatism.

For ease of presentation, this exercise is best conducted in one dimension. For Figures 7 and 8, the three groups vote as in Figure 4 except for a party identification term, which we assume (in the aggregate) correlates perfectly with the issue dimension and has a salience coefficient of 1.0 and a mean of 0.0. In other words, party identification equals the voter's

Figure 6. Candidate Equilibrium in Two Dimensions


+1.0
0.0
−1.0
−1.0
0.0
+1.0
A
B
C
Q*
Vote for X vs. Y at Q: A: .55 B: .26 C: .55 All Voters: .45
Vote for X vs. Y at Q: A: .13 B: .57 C: .62 All Voters: .44
Vote for X vs. Y at Q: A: .65 B: .60 C: .05 All Voters: .43

issue position: a_i. Arbitrarily, a_i represents support for Candidate X's party and $-a_i$ represents support for Candidate Y's party. For simplicity, the vote equation has no "intercept" (constant).

Like Figure 4, Figures 7 and 8 show a global equilibrium. But there are important differences. First, the new equation projects a winner instead of a tie. If both candidates stay at equilibrium, Candidate X wins more votes. For Candidate Y, the joint equilibrium position becomes the best expected losing vote. (Candidate X wins despite the even distribution of party identification, as an artifact of the lumpiness of the voter distribution into three groups. In general, the equilibrium vote will not be 50/50, because K_i will not necessarily sum to zero.)

Second, the vote functions for the two candidates are no longer symmetrical. Charting Candidate X's vote as a function of X's issue position while Y is at equilibrium requires one graph, and charting Candidate Y's vote as a function of Y's position while X is at equilibrium requires another. Each candidate has a better chance of winning the votes from supporters of the candidate's own party.

Third, the equilibrium outcome is no longer at the unweighted mean, zero. The party term makes group A voters the least

Figure 7. Joint Candidate Equilibrium with Party Identification: Candidate *X*'s Vote Function with *Y* at *Q**

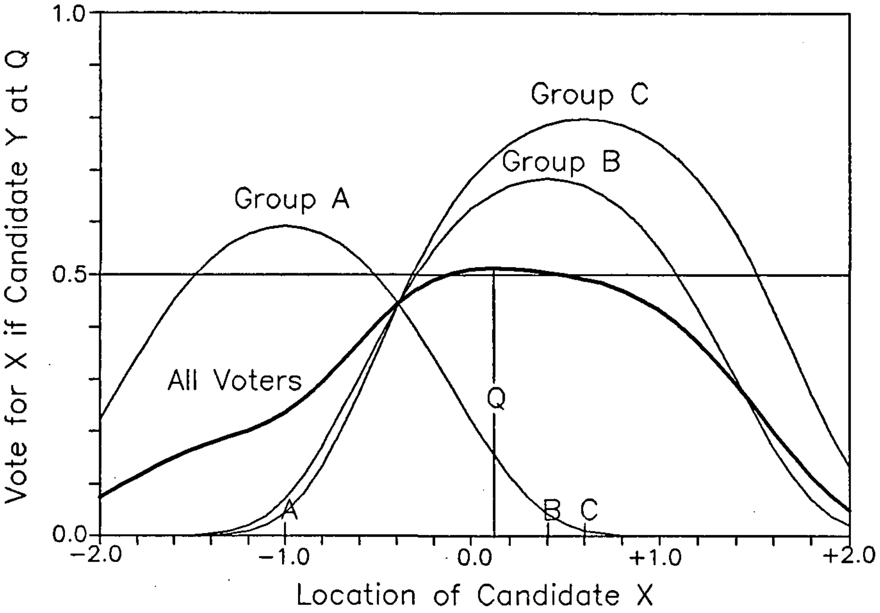
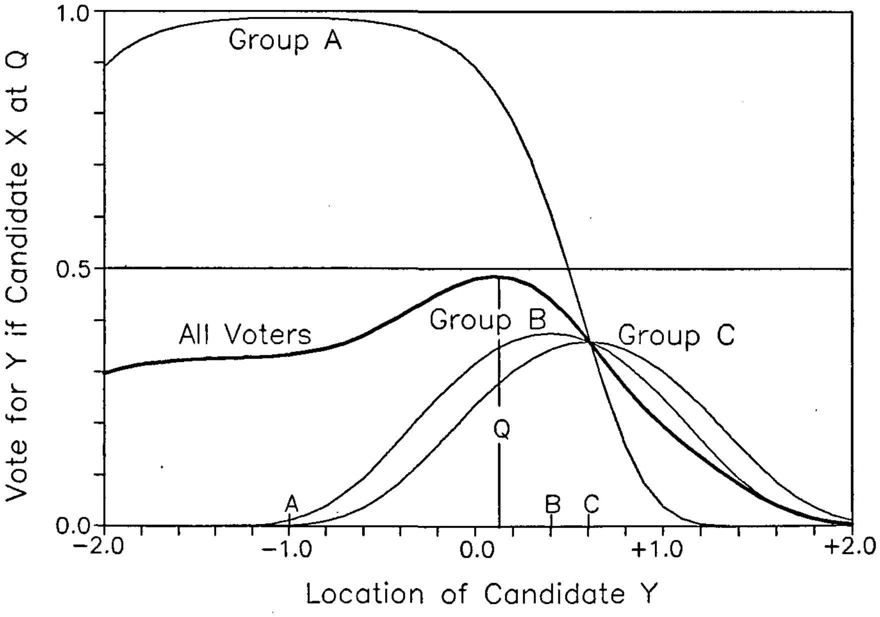


Figure 8. Joint Candidate Equilibrium with Party Identification: Candidate *Y*'s Vote Function with *X* at *Q**



marginal and group B voters the most marginal on nonissue grounds; and they are weighted accordingly (equation 7). As a result, equilibrium moves closer to the views of group B.

In this example, the equilibrium point moved in the direction of the winner's partisans. However, this result does not generalize, since the rightward movement of equilibrium is due to the skewed opinion division that was contrived. More generally, being on the losing side of an election would make a group's opinion *more* electorally powerful. Suppose, for Figures 7 and 8, we added a positive intercept to the vote equation, making Candidate X an even bigger equilibrium winner. This would make group A more responsive to candidate issue movement and groups B and C less responsive, moving the equilibrium point leftward again in the direction of the losing partisans.

In summary, when correlated with the issue dimensions, party identification (or any nonissue causes of the vote) will affect the location of the candidate equilibrium. As we have shown, the views of marginal voters weigh most heavily. In a one-sided contest, partisans of the losing party should find solace in the fact that their preferences weigh most heavily, even as their candidate loses.

Candidate Equilibrium and the 1988 Presidential Election

Our major interest should be whether candidate equilibria can be found amid the complicated issue space of an actual election. We shall search for a candidate equilibrium for the 1988 election, as represented by the 1988 National Election Study. First, we shall generate a general multivariate probit equation predicting the 1988 presidential votes of NES respondents. We use this equation to locate the local equilibria on eight issues for the NES respondents. (Candidate policy positions are assumed to correspond to the mean perceptions of voters among the NES sample.) Then we move candidates Bush and Dukakis around in issue space to illustrate the equilibrium properties of our solution. We place one candidate at the (local) equilibrium point on all issues and manipulate his opponent, with an eye for whether the local equilibrium is global.

The Probit Equation

We specify a "basic" probit equation as a composite of how behavioral researchers would model the 1988 presidential vote. The equation includes quadratic loss functions for each of eight issues. Each is chosen because respondents' issue positions and perceptions of candidate issue positions were estimated on the NES's familiar seven-point scale. Besides the eight quadratic loss functions, the equation includes respondent party identification and the respondent's net candidate evaluation in terms of the candidates' personal characteristics. Party identification is measured according to the usual seven-point scale. Net candidate evaluations are measured from summing (with appropriate valences) the open-ended likes and dislikes about the candidates. These two variables have a rich history of inclusion in empirical voter models (DeClercq, Hurley, and Luttbeg 1975; Goldberg 1966; Page and Jones 1979; Schulman and Pomper 1975). Table 1, column 1 presents the probit results of our basic equation.

Equilibrium

To estimate the (local) equilibrium point, we first find the weighted mean on each of the eight issues. The weight is determined from equation 7. Each case is weighted by the height of the normal curve at the point represented by the nonissue component of the probit equation. The resultant equilibrium is the intersec-

Table 1. Probit Equations Predicting 1988 Presidential Vote

Variable	Basic Equation (1)	Indirect Issue-Proximity Effects (2)	Varying Issue Saliences[a] (3)	Subjective Issue Proximities[b] (4)	Includes Nonvoters' Vote Preferences (5)
Respondent party identification	.439	.441	.453	.382	.453
	(.028)	(.028)	(.028)	(.030)	(.022)
Net candidate evaluation	.239	.239	.244	.167	.290
	(.031)	(.032)	(.031)	(.034)	(.028)
Issue proximities[c]					
Ideological identification	.035	.109	.075	.040	.026
	(.012)	(.012)	(.017)	(.011)	(.009)
Domestic spending	.036	.066	.058	.019	.036
	(.012)	(.012)	(.030)	(.009)	(.009)
Guaranteed jobs	.018	.020	.006	.002	.011
	(.009)	(.009)	(.019)	(.006)	(.007)
Health insurance	.015	.034	.077	.025	.002
	(.007)	(.007)	(.030)	(.008)	(.005)
Aid to minorities	.020	.025	.050	.017	.018
	(.010)	(.010)	(.040)	(.010)	(.007)
Womens' role	.023	.008	.042	.035	.008
	(.015)	(.015)	(.021)	(.010)	(.011)
Defense spending	.033	.067	.064	.060	.026
	(.009)	(.009)	(.015)	(.011)	(.007)
Dealing with Russia	.022	.003	.073	.016	.014
	(.022)	(.022)	(.175)	(.015)	(.017)
Intercept	.032	−.010	.075	.129	.078
Estimated R^2	.730	.730	.759	.842	.697

[a] Coefficients in col. 3 are normalized for a voter with a salience score of 1.00.

[b] For subjective issue proximities, respondent issue proximities are set to zero when they fail to rate both candidates on the 1–7 scale.

[c] Issue proximities are measured in the metric of quadratic loss.

tion of the eight weighted means. This basic equilibrium result is shown in Table 2, column 4. (Column 1 shows the unweighted voter mean; columns 2 and 3 show the mean perceptions of the Dukakis and Bush positions; columns 5–10 show some alternative equilibrium estimates from alternative models to be introduced.)

The basic equilibrium estimates are slightly to the left of the unweighted mean. The reason is that given Bush's edge on nonissue considerations, the more marginal (and therefore more heavily weighted voters) were slightly more liberal than the average. Thus, we have confirmation of the somewhat perverse principle that a losing campaign (on nonissue grounds) moves the candidate equilibrium in the direction of the losing candidate's supporters.

Among NES survey respondents, Bush defeated Dukakis .529 to .471. The simulations show that if both candidates were at equilibrium, Bush's margin would have been .519, which confirms Bush's slight

Table 2. Estimates of Candidate Equilibrium Positions for Eight Policy Issues, 1988 Presidential Election

				Equilibrium Locations	
Issue Dimension	Mean Voter Position (1)	Mean Perception of Dukakis (2)	Mean Perception of Bush (3)	Basic Model (4)	Indirect Issue-Proximity Effects (5)
Ideological identification	4.45	3.02	5.27	4.37	4.41
Domestic spending	3.99	2.78	4.46	3.92	4.00
Guaranteed jobs	4.61	3.20	5.19	4.61	4.58
Health insurance	4.02	2.92	5.15	3.96	4.02
Aid to minorities	4.48	3.23	4.94	4.48	4.44
Women's role	2.59	2.71	3.73	2.54	2.56
Defense spending	3.94	3.17	4.12	3.88	3.89
Dealing with Russia	3.76	3.35	4.12	3.70	3.71

	Equilibrium Locations				
Issue Dimension	Varying Issue Salience (6)	Subjective Candidate Location (7)	Including Turnout Effects (8)	For Dukakis, Bush Fixed at (3) (9)	For Bush, Dukakis Fixed at (2) (10)
Ideological identification	4.50	4.37	4.34	4.47	4.28
Domestic spending	4.16	4.01	3.94	3.99	3.84
Guaranteed jobs	4.22	4.57	4.59	4.71	4.59
Health insurance	3.96	4.08	3.93	4.02	3.88
Aid to minorities	3.64	4.47	4.51	4.58	4.50
Women's role	2.55	2.48	2.50	2.56	2.55
Defense spending	3.85	3.81	3.89	3.98	3.86
Dealing with Russia	3.47	3.67	3.71	3.73	3.69

Note: Each issue is scaled so that 1 equals the most liberal position and 7 equals the most conservative position.

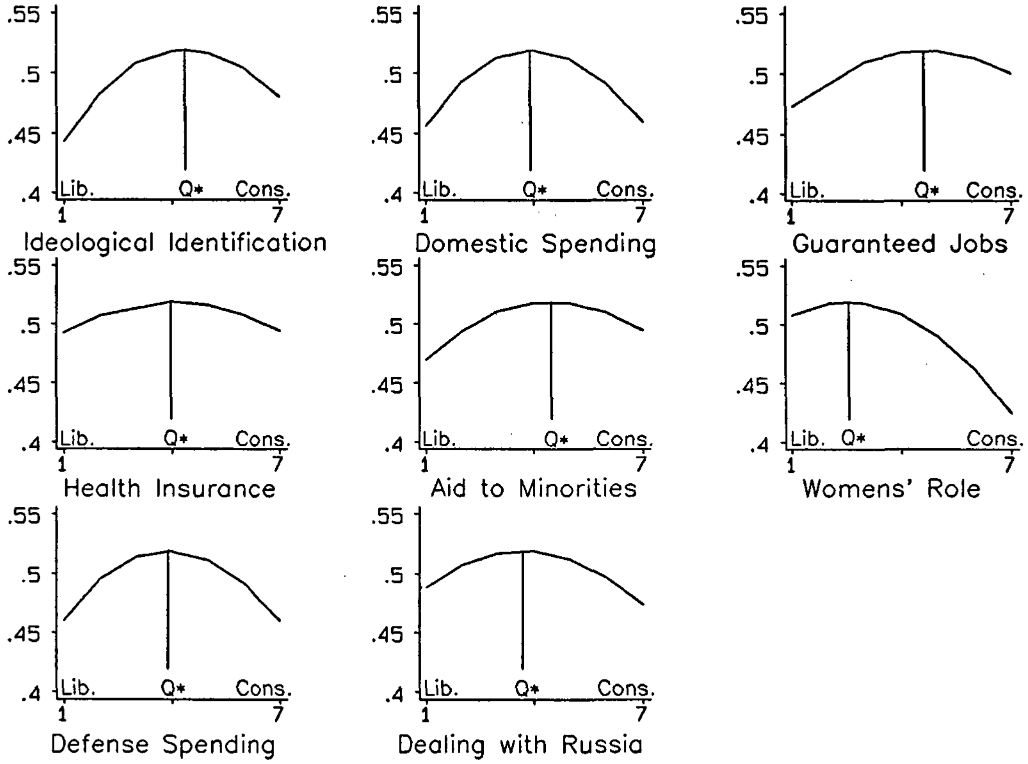
edge on nonissue grounds. Since Bush was a percentage point higher in actuality (.529) than at equilibrium (.519), he enjoyed a slight net advantage on the eight issues sampled. Of course, both candidates were far from the equilibrium point Q^*. With Bush at Q^* and Dukakis at his actual position, Bush would have won even more handily, with .555 of the vote. With Dukakis at Q^* and Bush at his actual position, Dukakis would have won narrowly with .511 of the vote.[2]

The Simulations

We simulated alternative 1988 election outcomes by manipulating the locations of candidate Bush or candidate Dukakis while placing the opponent at equilibrium. We harbor no illusions that candidates can actually manipulate their policy images in the manner we present. Rather, we ask what would happen if they could. Our simulations have candidates Bush and Dukakis moving so freely that we even consider outcomes with Bush to the left of Dukakis. In that spirit, we proceed.

Electoral responses to changes in candidate location were simulated by modifying the values of the candidate positions for the probit equation and recording the resultant change in the predicted vote. Each modification generates a probability of voting for Bush (Dukakis) for each

Figure 9. Bush Vote As Bush Moves off Q^* One Issue at a Time

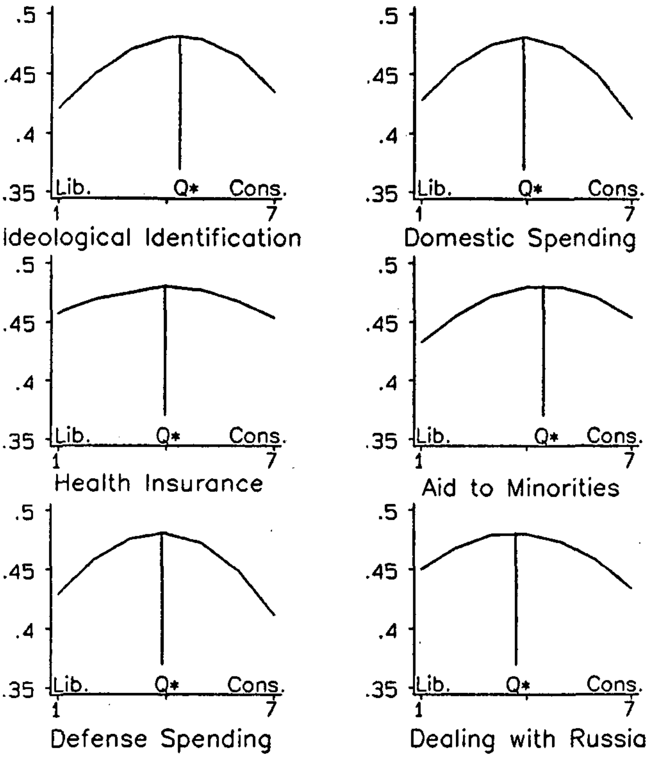


respondent. The predicted vote is simply the average of these probabilities. To simulate the equilibrium outcome, we set both candidates at the equilibrium position. As mentioned, the equilibrium result is a .519 Bush victory. The next task is to show that either candidate's vote declines if he moves away from equilibrium.

We conducted numerous simulations moving first Bush, and then Dukakis, off from equilibrium on one or more issue dimensions while keeping the opponent at equilibrium. In each instance, the manipulated candidate's maximum vote is at the equilibrium position. Moreover, each function is strictly concave within the permissible one-to-seven range of the scales, so that each additional increment away from equilibrium subtracts an ever-increasing margin from the candidate's vote. Although we obviously could not examine all possible movement in eight dimensions, there can be little doubt that the equilibrium point is a global equilibrium.

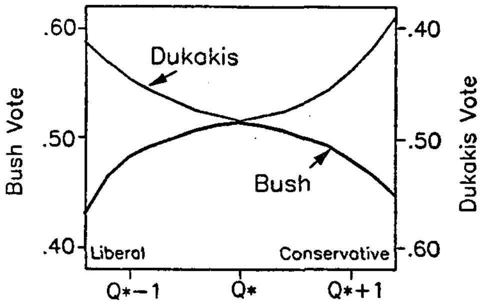
Figures 9 and 10 present the simulation results for movement off equilibrium one issue at a time for Bush and Dukakis, respectively. Small movements off equilibrium alter the predicted vote only slightly, since the electorate does not discriminate much between two candidates who are located very close together. Larger movements produce larger alterations in the predicted vote, with candidates falling down a slippery slope as they approach the edges of the one-to-seven scales.[3]

Figure 10. Dukakis Vote As Dukakis Moves off Q^* One Issue at a Time



We can also show the effect of movement off equilibrium for more than one dimension. Figure 11 shows, for each candidate, the consequences of moving from Q^* equal increments (as much as 1.4 units) left or right on all eight issues while the opponent stays at Q^*. (Movement is limited to 1.4 units from equilibrium to prevent candidates from going off the scale on some issues.) Movement from equilibrium on all eight issue dimensions, of course, has a greater impact than on one issue alone. Figure 11 shows Bush losing if he moves even two-thirds of a unit left or right of Q^* on all issues while Dukakis stays at Q^*. Meanwhile, if Bush stays at Q^* while Dukakis moves 1.4 units left or right on all issues. Dukakis' support collapses to about 40% of the vote. Showing movement much greater than 1.4 units away from Q^* would push candidates over the edge of some of the eight scales. But imagine Bush at 7 (the

Figure 11. Movement Left or Right on All Issues When Opponent Stays at Equilibrium



most conservative position) on all issues or Dukakis at 1 (the most liberal position) on all issues, while the other stays at Q^*. Bush at all 7s wins only .246 of the vote against Dukakis at Q^*. Similarly, Dukakis at all 1s wins a mere .247 against Bush at Q^*.

Summary

Given the 1988 NES survey respondents and the model of the vote as specified by our probit equation, we have located the candidate equilibrium point in 1988 issue space. This local equilibrium gives every appearance of also being a global equilibrium. The exact location of an equilibrium point depends on the specification of the beholder. Modify the voter equation in any way, and the supposed equilibrium point would change.[4] The important result is that by applying a multivariate probit model to the 1988 NES sample, it is possible to identify a theoretical eight-dimensional equilibrium point that neither candidate would have an electoral incentive to leave.

Alternative Models

So far, we have demonstrated a candidate equilibrium for one particular specification of the vote equation for the 1988 presidential election. We shall now explore the flexibility of the equilibrium solution, given some respecifications of the voter model for 1988 NES respondents. We consider four modifications of the voter model: to model indirect effects, to incorporate differential issue salience, to allow voters to vary in their candidate perceptions, and to adjust for effects of issue proximity on turnout.

Indirect Effects

Our basic model in the previous section allows only for direct (as opposed to indirect) effects of issue responses on the vote. Suppose, as some models suggest (Erikson 1982; Franklin and Jackson 1983; Jackson 1975; Markus and Converse 1979; Page and Jones 1979), the supposed "background" variables of party identification and candidate evaluations are actually endogenous. If so, a considerable flow of causality may run from issue proximity to the vote via party identification and candidate evaluations.

Given this sort of specification, when candidates weigh voters, they should take into account only the portions of voter partisanship and candidate evaluation not caused by issue proximities. Imagine, for example, the maximum indirect effects so that issue proximities are (unrealistically) totally exogenous to party identification and candidate evaluations. To conduct such an exercise, we regressed party identification and candidate evaluations on the eight issue proximities and employed the residuals as measures of these variables purged of the effects of issue proximities. The resultant probit equation predicting the vote is shown in Table 1, column 2. The coefficients for the eight issues represent the supposed combined direct and indirect effects of the proximities on the vote.

The new equilibrium points (Table 2, col. 5) are virtually identical to the raw means (Table 2, col. 1), rather than the weighted means of the basic model. The reason is that with issue proximities exogenous to party identification and candidate evaluation, voter marginality is equally distributed across issue positions. The more general intuition is to the extent that the control variables are themselves caused by issue proximities, candidates should discount the contributions of these variables to voter marginality.

Differential Issue Salience

Voting studies generally model the effect of issues on the vote without any adjustment for voters' weighing issues differently. The salience of an issue is implicitly the same for all voters—the probit (or regression) coefficient. Obviously, however, different voters may weigh each issue differently (Niemi and Bartels 1985; Rabinowitz, Prothro, and Jacoby 1982; Rivers 1988). Since the equilibrium model allows for variable b_{ij} terms, we can attempt to take differential salience into account for our 1988 example. The only difficulty is that the 1988 NES study (like its predecessors) is not generous with useful salience measures.

For the basic equation, the b_j salience weights are simply the unstandardized b coefficients. To allow individual variation in an issue's salience, we generated a new probit with each issue weighted not only by the equation's b term but also by an individual-specific salience measure generated from the open-ended responses. We based our salience measures on the open-ended likes and dislikes about the parties and candidates. From the NES codes for the likes–dislikes responses, we selected those that corresponded to each of the eight issues:

Issues	*Mention of*
ideological identification	ideological labels
domestic spending	proper role of government, general spending
guaranteed jobs	providing jobs, the work ethic, or helping the poor
medical insurance	medical/health care, costs, insurance, Medicare
aid to minorities	helping blacks, civil rights, busing, candidates and blacks
women's role	women's issues, abortion, ERA, women in politics
defense spending	defense, defense spending
dealing with Russia	Russia, communists, war and peace

We summed the weighted likes–dislikes responses for each issue area. Weights were 1.0 for a first mention, 1/2 for a second mention, 1/3 for a third mention, 1/4 for a fourth mention, and 1/5 for a fifth mention. Proportions of respondents with nonzero salience weights (at least one mention for the likes–dislikes) ranged from .61 for ideology to .05 for dealing with Russia.

These salience weights produce the probit equation shown in Table 1, column 3. Weighting for salience adds slightly to the estimated R-squared. The new (local) equilibrium points that result are shown in Table 2, column 6. These new equilibrium points are slightly different from the originals, reflecting a more liberal "issue public" for some issues (minority aid, guaranteed jobs) but more conservative issue public for others (ideology, government spending).

For the 27 issue pairs, the maximum correlation of salience weights is only .20. Thus, issue publics appear as intense minorities, generally different segments of the electorate on different issues. This is the type of electorate that might seem ripe for disequilibrium. Yet simulations based on the equation with differential saliences show the usual concave vote functions for the candidate who moves off equilibrium. Even allowing for individual salience weights, the derived (local) equilibrium position clearly is a global equilibrium.

Subjective Candidate Placement

In some voter models (e.g., Enelow, Hinich, and Mendell 1986) voters respond to their own subjective perceptions of candidate positions, rather than the mean perceptions. Table 1, column 4 presents the probit equation when perceived candidate positions are substituted for the mean perceptions. The estimated R-squared zooms to .842, although much of this "improvement" could be due to voter rationalization. Subjective candidate placements present a special challenge for the search for equilibrium. Define g_{ij} and h_{ij} as perceptual distortions of the positions of X and Y respectively, which remain constant for the ith individual as

Candidates X and Y modify x_j and y_j. Thus, the issue components of the voter equation take the appearance

$$J_i = \sum_{j=1}^{m} b_{ij}[(a_{ij} - y_j - h_{ij})^2 - (a_{ij} - x_j - g_{ij})^2],$$

where x_j and y_j represent "true" candidate positions (measured as mean perceptions) on the jth issue and g_{ij} and h_{ij} represent the deviation of the respondent perceptions from the true x_j and y_j position, respectively. If we substitute $(x_{ij} + g_{ij})$ for x_j and $(y_j + h_{ij})$ for y_j in equation 6, and with $x_j^* = y_j^*$, then

$$w_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp - \frac{(K_i + J_i)^2}{2} \tag{8}$$

$$= \frac{b_{ij}}{\sqrt{2\pi}} \exp - \frac{[(K_i + \sum_j b_{ij}\{(a_{ij} - y_j^* - h_{ij})^2 - (a_{ij} - x_j^* - g_{ij})^2\}]^2}{2}$$

$$= \frac{b_{ij}}{\sqrt{2\pi}} \exp - \frac{[(K_i + \sum_j b_{ij}(a_{ij} - x_j^*)(g_{ij} - h_{ij})]^2}{2}.$$

The intuition behind this complex equation is that candidates should discount voters who project their personal view onto their favored candidate.

The difficulty with equation 8 is that no longer does J_i reduce to zero at the joint $x_j^* = y_j^*$ equilibrium. Given this complication, solving for the x_j^*s presents a formidable but not impossible task of nonlinear optimization. We were able to estimate the w_js for the 1988 NES respondents by using an iterative process. We substituted the original x_js from our basic model into equation 8 as starter values for each issue and obtained new weighted means, substituting these into equation 8 again, and so on, repeating until the weighted means converged.

These alternative equilibrium points, shown in Table 2, column 7, are not much different from the originals of column 4. Remarkable is that these equilibrium points could be calculated at all. Despite the complexity of equation 8, selection of a model that allows voters to respond to their idiosyncratic perceptions of candidate positions does not prevent the location of an equilibrium position.

Candidate Positions and Voter Turnout

When candidates move about in issue space, they can affect not only the voter's candidate choice but also whether a citizen will vote in the first place. Spatial models may take into account voter abstention due to alienation (no candidates passing the threshold of acceptability) and due to indifference (the choice of candidates makes too little difference). The probabilistic response of turnout to candidate positions is a familiar topic (e.g., Hinich, Ledyard, and Ordeshook 1973; Enelow and Hinich 1984b). Here, we show how the equilibrium solution can be elaborated to take into account the effect of candidate movement on both vote choice and the decision whether to vote, simultaneously. We assume probabilistic abstention due to indifference toward the choice of candidates, plus other variables that independently affect the decision whether to vote.

If prospective voters abstain due to indifference, their probability of voting is a positive function of their absolute value of Z_i, or $|Z_i|$. Other variables may also affect nonvoting, such as respondent education and age:

$$P(T) = \frac{1}{\sqrt{2\pi}\sigma} \int_{-\infty}^{Z_i} \exp - \left(\frac{u^2}{2\sigma^2}\right) du,$$

where

$$Z_i' = c'_o + \sum_{k'} (c'_{k'} p'_{ik'}) + q|Z_i|)$$

and the $c'_{k'} p'_{ik'}$ terms represent the effects of k' additional variables (education, age, etc.) on turnout and q represents the effect of $|Z|$.

Taking turnout into account, Candidate X seeks to maximize plurality rather than vote percentage. The equilibrium solution is the usual intersection of the weighted means for *j* issues, but with the weights now defined as

$$w_{ij} = \bar{P}(T)_i^* \left[\frac{2}{\sqrt{2\pi}} \exp - \left(\frac{K_i^2}{2} \right) \right] + [P(X)_i^* - P(Y)_i^*] \left(\frac{qb_{ij}}{\sqrt{2\pi}} \right) \left(\frac{K_i}{|K_i|} \right) \exp - \frac{(Z_i'^*)^2}{2}, \quad (9)$$

where * indicates the value at equilibrium Q*.

In words, candidates should weigh voters according to the marginality of their vote decision times their likelihood of voting, *plus* the certainty of their vote decision times the marginality of their decision whether to vote. The first component reflects candidates' obvious benefit from wooing undecided voters who are likely to vote. The second component reflects candidates' more subtle benefit from wooing their less participatory partisans to the polls (by accentuating candidate differences) *and* from wooing the opposition's less participatory partisans away from the polls (by blurring candidate differences).[5]

To adjust for the effect of candidate positions on turnout, we first generated a vote preference equation based on voters and nonvoters alike. This is shown in Table 1, column 5. The absolute value of Z_i from this equation was included with education and age in a probit equation predicting 1988 turnout.[6] Predicted values of this equation comprise Z'. From all this, the new weights were generated, using equation 9. The resultant equilibrium estimates, based on all respondents rather than just voters, are in Table 2, column 8.

The equilibrium estimates with turnout taken into account are similar to the basic estimates of Table 2, column 4. The complicated weighting of equation 9 presents no bizarre wrinkles. In part, this is because aggregate turnout responds only sluggishly to candidate movements. Our simulations suggest that an equilibrium election with no issue differences between the candidates would have depressed turnout by only 1.6 percentage points. At the opposite extreme, a "polarizing" election with Dukakis at all 1s and Bush at all 7s would have boosted turnout by only 2.7%.

Summary

Using the 1988 NES data, a candidate equilibrium obtains quite generally under various specifications of the vote model. We have tried several complications. We have allowed for exogenous issue proximities, varying issue salience, subjective perceptions of candidate positions, and candidate positions to affect turnout.[7] In each instance, we obtain a set of equilibrium points across the eight issues.

Each of these variations produces a unique equilibrium solution, and it is not our task here to say which (if any) is correct. What is significant is that one can apply an implicit statistical model to one's spatial map of an electorate and arrive at an equilibrium solution. Indeed, our modeling of the vote should be viewed as modeling how candidates might view their electorates. In this sense, we model a two-candidate game. But it is a game with a theoretical solution.

Nonequilibrium Strategies

The reason candidates are advised to locate at the joint equilibrium position is that this strategy ensures the best vote percentage against the opponent's best position. If both are at the equilibrium location, each will only lose votes if he moves unilaterally. Of course, with probabilistic voting, the best vote percentage does not necessarily add to the prospect of victory. For instance, when candidate Dukakis achieves Q* in our election game, he is assured of no more than a graceful margin of defeat if Bush employed his best strategy. Candidate Dukakis may also make the empirical observation that other candidates seldom locate at their Q* position, so that Bush is unlikely to do so as well. For these reasons, our candidate Dukakis might choose to gamble on some nonequilibrium location that might be optimal given some notion of the range of possible Bush positions, although not Bush's equilibrium position. Potentially, this sort of thinking could add an element of instability to our election game.

When voting is deterministic on a single dimension (à la Downs), a candidate can gain votes by moving off equilibrium in the direction of a mislocated opponent when that opponent is fixed at some noncentrist position. Against a certain right-wing opponent, for example, the vote-maximizing strategy would be to move rightward almost the same distance, to maximize the number of votes closer to your position than your opponent's. This strategy does not work with probabilistic voting, however. Moving to a position only a shade closer to the center than one's fixed opponent would provide a nuance to which most real-world voters would not respond.

Our various simulations using the 1988 NES data suggest that when one candidate moves off Q*, his opponent should move only a small distance from Q* in the same direction; even then, the vote gain will be slight. To illustrate, we fixed first Bush, and then Dukakis, at their actual (mean perceived) positions on the eight issues, and sought the positions that would maximize the rival's vote. Each solution required a series of iterations.[8] These contingent equilibrium points (contingent on a fixed opponent) are in columns 9 and 10 of Table 2.

These ideal issue positions for Dukakis against actual Bush and for Bush against actual Dukakis are very similar to the original basic equilibrium estimates, tilting only slightly in the direction of the fixed opponent. Moreover, these slight modifications in issue position gain few additional votes. Had Dukakis employed his best issue strategy (column 10) against actual Bush, he would have won .522 of the vote, or .011 more than if he stayed at Q*. Against actual Dukakis, Bush would not have improved even a decimal point over his .555 victory with a Q* strategy. The joint equilibrium solution, therefore, is extremely robust in the sense that it is still close to the best strategy even for exploiting the weakness of an opponent who foolishly fixes his position away from equilibrium.

Conclusion

We have argued that candidates enhance their vote margin by placing themselves at the mean voter positions on the issues, with voters weighed differently according to their interest in an issue and according to the likelihood that they could change their vote intention. This conclusion may appear mundane in the sense that it only mirrors the commonly understood wisdom of practical politicians. What is remarkable about this conclusion is that it can be demonstrated by an application of spatial modeling.

Spatial models of the vote have emphasized the general absence of candidate equilibria. The implication of disequilibrium is that no candidate strategy is electorally superior to all others, so that any strategy, no matter how well conceived, can be beaten by another. But as others have shown, equilibrium can be restored by the assumption that voters respond probabilistically rather than deterministically to candidates' issue proximities.

We have extended this "probabilistic voting" result to the general case where voter motivations include *measured* nonissue variables, in addition to an error term. This theoretical result was then applied to the real-world electorate represented by the NES 1988 sample. For real-world elections with a relatively low degree of issue voting relative to other motivations, candidate equilibria do exist. Specific equilibrium locations were found for various plausible specifications of the vote equation. The seeming paradox is that complicating the vote model to include measured nonissue sources of vote decisions not only enhances the realism of the model but also induces the stability of candidate equilibrium that has been so elusive in simpler models of issue voting.

Appendix: Proof of Weighted Mean As Joint Candidate Equilibrium

Candidate X's task is to maximize his or her proportion of the vote [$V(X)$] as a function of Σx_j (X's policy positions) when Candidate Y chooses the Σy_j that is Y's best strategy. As we shall show, the solution is a joint equilibrium [Q*] where $x^*_j = y^*_j$ on all dimensions. Assume that Y is located at Q* and that Candidate X also is at Q* except for dimension *j*. Which position x_j maximizes Candidate X's vote percentage? We solve by differentiating $V(X)$ with respect to x_j and set the derivative to zero. This function is additive, so that

$$\frac{\partial V(X)}{\partial x_j} = \frac{1}{n} \sum_{i=1}^{n} \frac{\partial P(X)_i}{\partial x_j}.$$

By the chain rule for each voter,

$$\frac{\partial P(X)_i}{\partial x_j} = \left(\frac{\partial P(X)_i}{\partial Z_i}\right) \bullet \left(\frac{\partial Z_i}{\partial x_j}\right) \qquad \text{(A-1)}$$

Differentiating the two components of equation A-1,

$$\frac{\partial P(X)_i}{\partial Z_i} = \frac{1}{\sqrt{2\pi}} \exp - \left(\frac{Z_i^2}{2}\right)$$

and

$$\frac{\partial Z_i}{2x_j} = 2b_{ij}(a_{ij} - x_j).$$

After multiplying the two components and aggregating, we see that Candidate X's task is to find the value of x_j so that

$$\frac{\partial V(X)}{\partial x_j} = \sum_{i=1}^{n} \frac{1}{\sqrt{2\pi}} \exp - \left(\frac{Z_i^2}{2}\right) \bullet 2b_{ij}(a_{ij} - x_j)$$
$$= 0. \qquad \text{(A-2)}$$

Rearranging equation A-2,

$$\left[\frac{1}{\sqrt{2\pi}} \sum_{i=1}^{n} b_{ij} \bullet \exp - \left(\frac{Z_i^2}{2}\right)\right] \bullet x_j$$
$$= \frac{1}{\sqrt{2\pi}} \sum_{i=1}^{n} b_{ij} \bullet a_{ij} \bullet \exp - \left(\frac{Z_i^2}{2}\right)$$

Transposing, Candidate X maximizes the vote when

$$x_j = \frac{\frac{1}{\sqrt{2\pi}} \bullet \sum_{i=1}^{n} a_{ij} b_{ij} \bullet \exp - \left(\frac{Z_i^2}{2}\right)}{\frac{1}{\sqrt{2\pi}} \bullet \sum_{i=1}^{n} b_{ij} \bullet \exp - \left(\frac{Z_i^2}{2}\right)}$$

Equivalently,

$$x_j^* = \frac{\Sigma w_{ij} a_{ij}}{\Sigma w_{ij}}, \qquad \text{(A-3)}$$

where

$$w_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp - \left(\frac{Z_i^2}{2}\right).$$

By the same reasoning, Y maximizes his vote when X is at Q* by choosing

$$y_j^* = \frac{\Sigma w'_{ij} a_{ij}}{\Sigma w'_{ij}}, \qquad \text{(A-4)}$$

where

$$w'_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp - \left(\frac{-Z_i^2}{2}\right).$$

Since equations A-3 and A-4 are equivalent (substituting $-Z_i^2$ for Z_i^2 in the exponential term), the equilibrium point is the same for both Candidate X and Candidate Y. With both candidates at the same location when at equilibrium, the issue component of the vote equation (J_i) drops out, so that Z_i simplifies to K_i, the nonissue component. Substituting K_j for Z_j in equations A-3 and A-4, at equilibrium,

$$x_j^* = y_j^* = \frac{\Sigma w_{ij} a_{ij}}{\Sigma w_{ij}},$$

where

$$w_{ij} = \frac{b_{ij}}{\sqrt{2\pi}} \exp - \left(\frac{K_i^2}{2}\right)$$

which restates equation 7. Also, when $x_j^* = y_j^*$,

$$\frac{\partial^2 V(X)}{\partial dx_j^2} = \frac{-2\Sigma w_{ij}}{n} < 0,$$

verifying that Q^* is a maximum rather than a minimum.

Notes

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1. Probabilistic voting should not be confused with the separate matter of voters viewing candidate issue positions with uncertainty (Bartels 1986; Enelow and Hinich 1981). Probabilistic voting involves candidate perceptions of the electorate based on incomplete information of voter motivations. Voting with uncertainty involves voters choosing candidates with incomplete information about candidate issue positions.

2. Bush had a slight edge from candidate evaluations, while Dukakis had a slightly bigger edge from party identification. Bush's net advantage apart from issues, therefore, derives from a favorable intercept. It might be said that Bush owed his victory to variables omitted from the equation.

3. Figures 9 and 10 present wide-angle pictures that obscure the precision of the equilibrium "solutions." Suppose our camera zooms in on the exact equilibrium location. To take an issue at random, consider both candidates at the joint equilibrium, except that Bush has moved off the domestic spending equilibrium of 3.921. If both candidates are at this position on domestic spending, plus the equilibrium values on all issues, Bush wins .5193 of the vote. As Bush moves increments of .10 units to the right of 3.921 while he and Dukakis are both otherwise at Q*, Bush wins: .5192 at 4.021, .5184 at 4.121, and .5178 at 4.221. Similarly, as Bush moves increments of .10 units to the left, Bush wins: .5192 at 3.821, .5190 at 3.721, and .5187 at 3.621. We obtain this degree of precision by computing the probit coefficients to a sufficient number of decimal places to avoid rounding error in the computation of the voter weights for the estimate of Q*.

4. That we were able to compute local equilibria using equation 7 and then observe the estimated local equilibria act as equilibria in the simulations is a verification of the internal logic of the model. It says nothing, however, about the accuracy of the voter model chosen. Moreover, with spatial models, the central issue of accuracy is whether the model accurately represents the belief of the candidates about the electorate rather than whether the voter model itself is correct.

5. The equation is $Z' = -.84 + .29$ (high school) $+ .69$ (some college) $+ 1.04$ (college graduate) $+ .63$ (over 30) $+ .251\,|Z|$. Estimated $R^2 = .22$. All coefficients are significant. The *t*-value for $|Z|$ is 6.03.

6. A proof that equation 9 represents the appropriate weight when turnout is taken into account is available on request.

7. Still another variation is to model voter utilities as functions of the absolute values of candidate–voter distances (the "city block" model) rather than quadratic loss. The quadratic loss model dominates the theoretical literature and is typically the model of choice in empirical studies (e.g., Enelow, Hinich, and Mandell 1986; Markus and Converse 1979). But the city block model is sometimes employed (e.g., Page and Jones 1979). With the city block model and probabilistic voting, the equilibrium solution is the weighted median rather than the weighted mean (Hinich 1977, 1978). We intend to explore the city block model in a subsequent paper.

8. With the opponent positions fixed, we first set the variable candidate at the basic weighted means (Table 2, column 4) and used the weighting formula of equation 6 to obtain a new set of weighted means. We continued to substitute estimates of the weighted means until the estimates converged, after three iterations. These are the estimates of Table 2, columns 9 and 10.

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IS WORK SPECIAL? JUSTICE AND THE DISTRIBUTION OF EMPLOYMENT

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I investigate from an egalitarian welfarist standpoint the issue of whether state-guaranteed employment should be offered as a matter of right to able-bodied poor citizens who suffer chronic or persistent unemployment. An affirmative answer is defended. One consideration supporting this right to work is that provision of transfer aid in the form of jobs rather than cash screens "nonneedy bohemians" from the class of beneficiaries. The main conclusion of the essay is defended from one objection based on efficiency wage models of the labor market and from another based on the alleged impossibility of maintaining the self-respect and self-esteem of able-bodied individuals who fail to earn their own keep independently in a market economy.

The plight of the unemployed elicits a hostile response from some who suspect that the "involuntarily unemployed" either have freely chosen their situation or have been plunged into it by their own faulty conduct. This attitude may be as common today as in 1884 when Herbert Spencer gave expression to it in these words:

> On hailing a cab in a London street, it is surprising how frequently the door is officiously opened by one who expects to get something for his trouble. The surprise lessens after counting the many loungers about tavern-doors, or after observing the quickness with which a street-performance, or procession, draws from neighboring slums and stable-yards a group of idlers. Seeing how numerous they are in every small area, it becomes manifest that tens of thousands of such swarm through London. "They have no work," you say. Say rather that they either refuse work or quickly turn themselves out of it. They are simply good-for-nothings, who in one way or other live on the good-for-somethings—vagrants and sots, criminals and those on the way to crime, youths who are burdens on hard-worked parents, men who appropriate the wages of their wives, fellows who share the gains of prostitutes; and then, less visible and less numerous, there is a corresponding class of women." (1909)

A currently widespread opinion agrees with Spencer that able-bodied persons who are neither independently wealthy nor supported by a spouse morally ought to work for their living. The *work ethic* is the idea that there is such an obligation to work. I shall consider the issue of the distribution of employment from the standpoint of a broadly egalitarian welfarist norm of social justice. On its face, the stern moralism of the work ethic appears to conflict with any version of welfarism, which holds that the proper business of the state is to help each person live a satisfying life according to that person's own conception of a satisfying life; but this appearance turns out to be deceptive. The main questions for this essay are (1) Does justice require that the state guarantee work to all able-bodied adult citizens? Is there a right to work? (2) In particular, if the state provides welfare benefits to able-bodied needy citizens, should this package of benefits include the opportunity to

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work? I shall assume that these questions pertain to an economy with a labor market. Firms (whether privately or publicly owned) are free to offer employment on terms of their own choosing, and individuals are free to accept or reject any firm's offer. I do not assume that the labor market is fully competitive (i.e., that the labor market clears).

Egalitarian Welfarism

Egalitarianism means giving more weight to securing gains and avoiding losses for the worse-off. *Welfarism* means taking rational self-interested preference satisfaction as the measure of the good life for individuals. Specifically, *egalitarian welfarism* holds that the moral value of a gain in welfare is directly proportional to its size and inversely proportional to the welfare level of its recipient. In other words, the worse off in welfare a person is, the greater the moral value of improving that person's welfare by a single unit. (This is the "weighted utilitarianism" advocated in Weirich 1983.) Obviously, this view assumes the theoretical possibility—not necessarily the practical feasibility—of cardinal interpersonal welfare comparisons. Sometimes, for simplicity, I shall consider the implications of *maximin* welfarism: one ought always to act and to arrange institutions so as to make the worst off in welfare as well off as possible.[1]

Sometimes competent individuals by negligence act imprudently in ways that reduce their own welfare. Sometimes individuals voluntarily choose to sacrifice their own welfare for the sake of others or for some valued cause. In both cases it is implausible to hold society responsible for rectifying the welfare loss to the individual that is the expectable consequence of that person's own voluntary or negligent choice. Hence, egalitarianism is better construed as holding society responsible for providing appropriate *opportunities* for welfare to individuals, rather than determining the actual welfare levels that individuals reach. Egalitarian welfarism so understood is the norm of equal opportunity for welfare. Defense of this norm lies beyond the scope of this essay, although I have defended it elsewhere (see Arneson 1989, 1990a, 1990b, 1990c; Cohen 1989).

Beyond the circle of committed egalitarians, the discussion should be of interest to anyone who is curious as to what a commitment to egalitarian ideals would entail. Moreover, many persons agree to *some* degree with the judgment that it is morally a matter of greater urgency to "give the little guy a break" than to help the already-well-off.

The Able-Bodied

For the purposes of this discussion, an *able-bodied* or *potentially employable* person is one for whom a full-time job could be devised such that that person, set to work at the job, would be better off and the job would be on balance socially beneficial, taking into account all costs and benefits due to the job, including opportunity costs. A job is on balance socially beneficial if the total of the utility that accrues to gainers from it exceeds the utility total that is lost by those who are put at a disadvantage by it.[2] Notice that according to this definition a smart quadraplegic might qualify as able-bodied, while a person who is strong as an ox but prone to absenteeism and quarreling with associates might be non-able-bodied. Notice also that if there are several unemployed persons, the notion of *able-bodied* just defined is ambiguous in that according to it any one of the unemployed persons taken separately might be able-bodied even though the set of unemployed persons regarded collectively is non-able-bodied. I shall assume in the situation considered that this ambiguity does not come into play.

Troublesome Unemployment

Individual joblessness is not invariably a proper object of social concern. Talented individuals may choose to become mendicant friars. The husband of a highly paid doctor or lawyer may by joint family decision elect the role of househusband. Millionaire heirs and heiresses may have no desire whatsoever for a job.

Such examples would not register in unemployment statistics. But consider construction workers who are laid off seasonally in states with cold winters, this seasonal layoff being a stable feature of the trade known to all entrants. Or consider that college students seeking summer work will be reported as unemployed in official unemployment statistics.

Unemployment that is chosen in order to facilitate job search can be productive in promoting a better match of individuals to jobs that suit them. Reasonably undertaken job search unemployment is nonproblematic from an egalitarian standpoint except insofar as government ought to be supplying more information to disadvantaged persons regarding available job opportunities so that they can swiftly obtain the best jobs they are qualified to perform.

Some unemployment is voluntary in the sense that given certain options—including at least one employment option—a person chooses freely not to take the employment option. Voluntary unemployment is morally bad according to egalitarianism if the set of options that includes a rejected employment option is too meager. If I choose unemployment rather than work under hellish conditions for extremely low wages, the "voluntary" character of my joblessness does not rule out the possibility that society treats me unjustly by failing to offer me better job options.

Even if unemployment is voluntary in the weak sense just described, it might be involuntary in what we might call the economist's sense (Akerlof and Yellen 1986, 11). In this sense, an individual is *involuntarily unemployed* who wishes, and is qualified, to take employment at prevailing wages but cannot obtain such employment. In other words, unemployment is involuntary when the supply of labor and the demand for labor are not equating: the labor market is not clearing.

Not all involuntary unemployment in the economist's sense is a priority concern for an egalitarian theory of distributive justice. If a lawyer working for a top-drawer firm is fired and wants to take work at another top-drawer firm at the going rate and declines employment at a second-level firm, this state of affairs is not a black mark against the society's egalitarian credentials. To a first approximation, involuntary unemployment (in the economist's sense) is morally bad from an egalitarian standpoint only when it afflicts the disadvantaged—those badly off in lifetime prospects for welfare. (For practical purposes, we can identify the disadvantaged with those who are far below average in wealth and income.) But from an egalitarian standpoint unemployment among the disadvantaged is especially offensive when the unemployed desire certain jobs and could perform them satisfactorily but cannot get such jobs.

Some bouts of unemployment are small potatoes: they do not add up to much disutility. The ill effects of joblessness on the jobless person tend to increase with time spent out of work (Kelvin and Jarrett 1985, chap. 2; also Sinclair 1987, 34–36). So it is sensible to focus concern on unemployment that is either persistent (the person suffers a single, long spell) or chronic (the person suffers several short spells of joblessness within a given time period, so that the cumulative negative impact on welfare is considerable).

The unemployment that is primarily troublesome from an egalitarian standpoint (1) afflicts the disadvantaged, (2) is either persistent or chronic, and (3) if voluntary, is freely chosen from a choice set that is too meager. Unemployment is secondarily troublesome if it either afflicts individuals whose high welfare prospects make them a lesser-priority concern or indirectly causes welfare loss to the disadvantaged. An example of such indirect loss would be unemployment among affluent workers when there is a redistributive tax and transfer program in place so that unemployment among the affluent lessens the tax funds that go to the nonaffluent citizens.

Why Offer Employment?

From the standpoint of egalitarianism, the question arises why provision of employment opportunities should have any special status in welfare policy. Suppose that it were proposed to implement a maximin version of egalitarianism (make the worst off as well off in welfare as possible) by policies of redistributive taxation and transfer set in the context of a market economy.[3] The welfare case for transfers of money from rich to poor individuals is obvious.

Does egalitarianism imply a commitment to redistribution of goods other than money? If so, how exactly does it imply it? The worst-off group[4] will surely include many who are chronically unemployed and others who succeed in gaining employment only intermittently—and then it is low-wage, tedious, and insecure employment. Among the chronically unemployed, many will be averse to work—at least to the work they are able to get. To be somewhat simpleminded about it, we can say that a market economy tends to distribute the most pleasant, interesting, and highly paid work to those with high "ability."[5] Low "ability" restricts one's employment options to (at best) the worst jobs. On the face of it, then, it is unreasonable to insist that the package of welfare benefits to be supplied to the worst-off by the just institutions of an egalitarian society must include guaranteed employment. Many of the worst-off will be ill suited to work and undesirous of it. Provision of jobs appears to be a costly and inefficient way of improving the welfare of the worst-off. If the "benefit" of guaranteed employment is mandatory, the policy looks to be cruel as well as inefficient. But despite these appearances, a society committed to a humane and sensible egalitarianism would guarantee the opportunity for decent employment to able-bodied members of the class of worst-off citizens.

Several reasons support the idea that egalitarian norms, combined with correct empirical assumptions, imply state-guaranteed employment. One consideration is political feasibility. Even if cash grants worked more efficiently to boost the utility of disadvantaged persons than provision of employment, a program of state-guaranteed employment might be uniquely palatable to voters in modern democracies. Opinion surveys routinely show that the majority of citizens harbor grave qualms about the wisdom of a state policy of handing out unearned income to the able-bodied but support programs that offer employment opportunities to able-bodied needy persons. But I further believe that the policy of state-guaranteed employment beats cash transfers even if the latter were politically tolerable to the majority of voters.

Another consideration is that the standard economic case for provision of transfer aid in the form of money, rather than specific goods, does not apply to the benefit of employment. The standard argument is that any individual should be presumed to know his or her own interests and the best means of advancing them better than others who might be tempted to override individual judgment. Given this presumption, individuals will attain higher welfare if provided the maximally flexible asset of money rather than an equivalently priced specific bundle of goods. But individuals cannot use money to purchase employment. I cannot purchase the job of my choice, nor do labor markets work in such a way that I can bid down the wages of a job that I want (which I could more easily afford to do if given a cash transfer). So if decent employment is generally a great benefit for an individual, it is not a benefit that cash bestowed by the government can assure.[6]

But this only shows that it would be misguided to assume that people with adequate cash who are rationally prudent must have all the employment they want, given the expected costs and benefits of employment. If one judged that for some, but not all, unemployed persons, employment (of the sort the state should provide according to egalitarianism) would be a benefit, the solution might be to offer a redistributive package of benefits to the unemployed disadvantaged that included a choice of job opportunities or a comparably valued cash transfer. The alternative policy of providing redistributive transfers solely in the form of employment opportunities would be justifiable only if employment should be deemed beneficial to (almost) everyone and if some of the disadvantaged would not handle the choice between cash and work in a rationally prudent manner.

Paternalism and the Value of Employment

Society might judge that some people will tend to shun work if they are given no-strings-attached cash transfers that provide adequate income and that despite their choices, these people would be better off working. To this extent, the case for providing employment opportunities rather than cash is an instance of the case for in-kind aid in general rather than aid in cash (see Thurow 1976; see also Van Der Ween and Van Parijs 1986 and Van Parijs 1990). Voters might prefer to give the poor food stamps (redeemable only for nutritious food) rather than cash (which might be spent on beer or silk stockings). There are two possible sources of this voter preference, one morally respectable and one highly questionable. The respectable source would be a paternalistic judgment that some poor people are not able to make good self-regarding decisions and will be better off if their options are limited. The questionable source would be a meddlesome self-interested preference of the voters that poor people should lead a thrifty cautious life rather than life in the fast lane. From an egalitarian welfarist perspective, at least, this is questionable because the point of state intervention and redistribution is to make the disadvantaged better off, not to benefit the already-well-off citizens who are taxed to pay for redistribution. Leaving aside the questionable motive, we need to consider whether such a paternalistic judgment is reasonable. In general, why suppose that earning one's living at the sorts of jobs that the disadvantaged are likely to be able to get is more advantageous than getting one's living without incurring employment obligations?

In considering the badness of unemployment, I restrict attention to its nonpecuniary disadvantages, since its pecuniary disadvantages can be remedied by cash transfers.

Perhaps it is true that in existing societies increased leisure[7] is a benefit to those who are employed but a source of disutility to the unemployed. Sociologists report that the unemployed are preoccupied with time and experience themselves as unable to use their time productively (see Kelvin and Jarrett 1985, chap. 5). But, of course, the unemployed represented in these studies are suffering income loss along with job loss. Leisure and money are complementary goods. The combination of joblessness, much leisure time, and lack of money might produce unhappiness that for many of the unemployed could be remedied by cash transfers. The limits of this remedy would then simply be the generosity of the taxpayers.

Among the possible nonpecuniary benefits of employment are the satisfaction of earning one's own keep; the intrinsic satisfaction of skillful task performance; praise for the quality of one's work from coworkers, customers, or supervisors; friendly and collegial relations with people regularly encountered in the course of the job; the knowledge that one is significantly benefiting others; escape from unwanted freedom owing to the fact that the job schedule imposes structure on the flow of time (as discussed in Elster 1988); and autonomy or desired freedom to organize one's tasks as one prefers. But of these sundry benefits, all but the first are also obtainable from a stable commitment to unpaid volunteer labor for a cause, to avocational associations and activities, or to devotion to family life and the care of relatives. So the nonpecuniary benefit of earning one's living (important as means to self-esteem) provides the most straightforward and promising basis for an affirmative answer to the question whether able-bodied persons are always better off employed rather than unemployed, other things being equal.

Persistent unemployment or frequent bouts of unemployment erode one's sense of self-efficacy and self-esteem (i.e., confidence that one's plan of life is worthwhile and that one is competently pursuing it).[8] The stigma of long-term unemployment is due partly to the presumption that failure to succeed at market activity signals personal incompetence and partly to the belief that the long-term unemployed person wishes to be a parasite on the rest of society. The persistently unemployed agent looks to be incompetent or immoral or perhaps both. Being afflicted with subpar self-esteem saps one's energy to pursue life plans and lowers the rate at which one can transform goods consumed into personal utility. It might then be argued that possession of adequate self-esteem is prerequisite to human flourishing and that for able-bodied individuals successful performance of the duties of a decent job is prerequisite to gaining and maintaining adequate self-esteem. Egalitarianism will then prescribe provision of the opportunity for decent employment to the worst-off citizens in order to ensure access to self-esteem.

To my mind this argument has some force but is unsound as stated and must be qualified. For one thing, there are ways to gain self-esteem other than job performance. Individuals can be dedicated to pursuing avocations, cultivating friendships, carrying out duties of family life in exemplary fashion, attaining some virtue or recognized excellence of achievement, or working in free time for a cause one respects. Any of these and many other projects can be sources of self-esteem obtainable independently of the quality of one's employment and on-the-job experiences. The relationship of getting a job and executing its duties to attainment of self-esteem is not anywhere near so tight as the nonpecuniary benefit argument supposes. Perhaps the most that can be said is that for most people conspicuous failure to get and keep decent employment will pose a threat (greater or lesser in magnitude) to their self-esteem.

One might speculate that the truly disadvantaged—those worst-off in welfare among the sporadically employed—will tend to lack the personal qualities that would enable them to use the expansive leisure afforded by unemployment to engage in activities and associations that will elicit praise from others and bolster their self-esteem. In that case the connection between gainful employment and self-esteem would be tighter than the nonpecuniary benefit argument supposes. But against this speculation another should be set. Jobs at the bottom of the status hierarchy are badges of failure in a competitive society. The more efficiently and fairly markets work in a reformed state of society, the more reliably would working at an undesirable job signal that the jobholder has undesirable personal traits. Faced with the threat to one's self-esteem posed by the unattractiveness of the jobs one can get, an individual might reasonably decide to drop out of the rat race if that was an option. Exit from the labor force might protect one's self-esteem if the only option is working at a job utterly lacking in prestige.

Another weakness in the self-esteem argument formulated so far is that the linkage between self-esteem and employment that it posits, if truly present in a society, is forged by cultural beliefs that could be changed and perhaps ought to be changed. Most people in modern competitive market societies accept the work ethic and believe that the failure of an able-bodied person to earn his keep is degrading. Most people also put a high value on job performance that issues in market success. Market competition tags some people as competent and others as incompetent; and most people who notice these tags embrace the judgments they announce. But perhaps an egalitarian norm ought to reject this way of thinking.

Does Egalitarianism Support the Work Ethic?

Suppose that society successfully inculcates in its members the moral belief that citizens ought to be self-supporting. Assume, further, that the prevalence of this work ethic on balance increases people's labor force participation and thereby increases national income. There is then more of a surplus to be taxed than would otherwise be available. This could potentially be funneled into redistribution that aids the worst-off (though whether such funneling would occur is another question, and it might be that the prevalence of the work ethic also causes citizens to be more stingy in voting for redistribution than they would otherwise be).

If the job any individual gets is more likely to yield net advantage, the higher that individual's ability, then the work ethic is likely to cause more low-ability than high-ability persons to take jobs to their disadvantage. On the other hand, the work ethic might tilt the scales in favor of a decision to seek and keep employment by many disadvantaged persons who would otherwise shun work to their long-run detriment.

With a work ethic in place, some individuals will be unable to satisfy it, and there will surely be considerable overlap between this group and the class of individuals worst off in welfare. It will either trouble these individuals that they cannot satisfy the work ethic or it will not. If (as is perhaps likely) their inability or lack of opportunity to fulfill a revered social obligation causes them grief, to that extent, the prevalence of the work ethic is cruelly afflicting the already-afflicted. It would be better in principle to restrict the scope of the work ethic—if indeed, it is good according to egalitarianism to foster this ethic at all.

The principled hostility of egalitarian welfarism to the idea of an across-the-board obligation to work can be exhibited by assessing the nineteenth-century socialist slogan, *From each according to his ability, to each according to his needs!* From an egalitarian welfarist standpoint, the problem with the ideal of bringing the economy into conformity with this norm is that (even if needs are interpreted as urgent rational preferences), people with the same ability have different degrees of liking for work or aversion from it. Harriet and Richard have equal ability; but Harriet finds it deeply satisfying to work to the limit of her ability whereas Richard hates any kind of productive employment that taps his ability. If both Harriet and Richard are required under an ideal socialist dispensation of the future to work according to their abilities and both receive in return goods that enable them to attain the same proportionate degree of satisfaction of needs, Richard is worse off than Harriet in terms of welfare. A scheme that is otherwise simmilar but requires Harriet to work more and Richard to work less would seem to be morally preferable according to the standard of egalitarian welfarism. If Richard's aversion from work is sufficiently intense, egalitarian welfarism will approve the outcome in which Harriet works and Richard does not and the proceeds of Harriet's labor are shared between them so as to yield equality of welfare.

This shows that the question is too narrowly posed if framed just in terms of the consequences for the worst-off of the prevalence of a work ethic. Alongside the obligation to work there are various pro-work attitudes and desires that might be promoted. Such values as pride of craftsmanship and the satisfactions of hard work reinforce the work ethic. Obviously, if work must be done, it is better from a welfarist standpoint if people are socialized to like work rather than hate it. But from an egalitarian standpoint, the glorification of work can cruelly affect the welfare prospects of low-ability individuals, who will have great difficulty finding work that they like and that will earn them pay or praise. Again, some relaxation of the standard as it applies to those who are harmed by it or else some compensation for this harm appears to be called for.

So long as the work ethic prevails in society, unemployment strongly tends to reduce people's self-esteem. Given the centrality of self-esteem to the prospects of living a good life, perhaps opportunities for decent employment should be guaranteed by the state to those among the disadvantaged who experience persistent difficulty obtaining it. But if the work ethic leaves the worst-off worse off than they otherwise could be, an egalitarian will look for ways to make progress toward its demise.

An Argument for Guaranteed Employment Rather Than Guaranteed Income

High-ability persons tend to derive more pecuniary and nonpecuniary benefit from work than low-ability individuals. But the ability to achieve market success is to some extent correlated with the ability to make use of one's leisure time in gratifying ways, even with a low stock of resources that are complementary to leisure. Thus, from the facts that more-able persons (1) gain more utility from work on the average than less-able persons and (2) can pick from a wider and richer range of employment opportunities, it does not follow that the class of chronically unemployed and marginally employed persons will consist solely of very-low-ability persons. This group will also contain some high-ability persons who happen to have a pronounced taste for leisure over work. I shall call this group the *nonneedy bohemians.*

This heterogeneity within the class of unemployed and barely employed persons suggests an objection to a tax-and-transfer scheme that provides a direct subsidy to the income of very-low-income persons, the size of the subsidy being dependent only on how low one's pretransfer income is. The subsidies under such a negative income tax scheme would not go just to those who are badly off by welfarist standards. Some of those with low income will be high-ability persons who could get high-paying employment but do better for themselves working little and enjoying lots of leisure. These individuals are already enjoying a high level of preference satisfaction, so there is no egalitarian case for boosting their income by government transfer.

Our aim is to help those whose opportunities for welfare are far below average. State provision of low-skill jobs at the minimum wage to the chronically unemployed has the merit of benefiting the members of the unemployed who are subpar in welfare opportunities while offering little or no benefit to the nonneedy bohemians.

The argument just given tries to establish guaranteed employment rather than guaranteed income as a good second-best policy, given the inability of the government reliably to distinguish the truly disadvantaged (those with low skill and low welfare) from the nonneedy bohemians (those with high skill levels and high welfare) among the class of those who display weak attachment to the labor force. This argument leaves open the possibility that if we could distinguish the disadvantaged from the nonneedy bohemians who are mixed together in the class of the long-term marginally employed, it might be better to offer the disadvantaged cash income grants that are *not* conditional on their willingness to take such employment as might be offered by the state; that is, whether employment is invariably a good for the worst-off or not, a policy of offering jobs rather than cash transfers to the chronically and persistently unemployed might be the best feasible egalitarian welfarist policy.

This argument needs another qualification. Among the class of persons who are both disadvantaged and unemployed, some will have a pronounced taste for leisure, so that their welfare prospects would not be enhanced by egalitarian provision of employment opportunities. These persons can be labeled *needy* bohemians. From an egalitarian welfarist standpoint, the policy of offering aid to the disadvantaged in the form of job opportunities rather than cash transfers is a good idea only if the desirable effect of screening nonneedy bohemians from the class of beneficiaries of the policy outweighs the undesirable effect of screening out needy bohemians.

No doubt the undercurrent of resentment of government transfer programs to aid the poor in some contemporary industrial democracies is for the most part incompatible with egalitarian principles of any stripe. But it may be worth noting that some members of the working poor (and their advocates) might reason as follows: "We do our best to stay employed even though the jobs we can get are gruelling and unsatisfying, because we feel morally bound to obey the work ethic. But many of those who have slight attachment to the labor force and who are stable recipients of government transfer aid openly flout the work ethic and have a pronounced taste for leisure, so they are better off than we are. It is not fair that the government should tax us so as to bestow aid on people who are thereby rendered better off than we are even though they are morally less deserving." Provision of job opportunities, rather than cash transfers, to the able-bodied forestalls this complaint.

The argument from the inability of society (governmental agencies) reliably to draw distinctions that in principle are morally important holds more generally. On the level of abstract principle, egalitarianism would imply promulgating the work ethic with reduced scope, so that the obligation to work would apply only to individuals whose compliance with it would promote good consequences in terms of the welfare of the worst-off. In its application to the worst-off, the work ethic should be binding only on those who are made better off by it. For some of the worst-off, the paternalistic imposition of an obligation to work improves their expected lifetime welfare prospects; other members of the worst-off should be excused from the obligation. Thus, an egalitarian version of the work ethic would be that people are morally obligated to contribute to society by working at some regular employment unless both (1) one's welfare prospects place one in the worst-off class and (2) one's character and circumstances are such that being under an obligation to work would damage one's welfare prospects (without generating offsetting benefits for some other members of the worst-off class).

But (to understate the point) this egalitarian work ethic is not a feasible guide to policy. Its administration presupposes a capacity for fine-tuned discriminations that is not to be found. If I identify myself as one of the worst-off, there is no reliable procedure I could follow in order to decide whether I am one of those persons who ought to be excused from the obligation to work. Nor could government agents alleviate my uncertainty on this point. Thus, even if on grounds of principle egalitarians reject the simple work ethic, in practice they must also reject the egalitarian work ethic just sketched. A feasible egalitarian version of the work ethic would be that people ought to do what they can to support themselves unless the attempt to be self-supporting causes one extreme, unavoidable misery. Those who cannot work or cannot work and lead a good life are excused from this obligation. (They are the non-able-bodied.)

The upshot of this discussion is that provision of job opportunities, rather than cash transfers, to the able-bodied is recommended as a second-best policy for the real world in which the best is unattainable. For various reasons, we can be reasonably sure about the nature of second-best policy in this respect even if we are in the dark about the best.

The moral case I have urged concerns the provision of employment opportunities to the perennially unemployed for the purpose of aiding the disadvantaged, the worst-off. Nothing has been said concerning employment policy with respect to the rest of the population. But since unemployment of significant duration tends to be concentrated among those who are below average in prospects for what we reasonably take to be good proxies for welfare, egalitarianism will favor full-employment policies. Moreover, since boosting the employment prospects of the least-qualified cannot proceed very far without boosting the employment prospects of the better-qualified, concern for the worst-off begets a strong instrumental concern for full employment. (See Ginsburg 1983 for a discussion of full-employment policies.) Full employment becomes a lesser priority from an egalitarian standpoint when the labor force is divided into noncompeting groups, so that a tight labor market fostered by government policies can coexist with a loose labor market among better-off workers who will never be candidates for low-skill jobs.

The conventional wisdom in Western democracies tends to reject the right to work as here envisaged. Probably the reason modern democracies do not guarantee work to all who are willing to take it is simply that a genuine full-employment policy would favor the interests of the perennially unemployed but run counter to at least the perceived interests of nearly everybody else. It is commonly thought that in capitalist economies subject to Keynesian dynamics, a serious attempt by government to ensure full employment would create a politically unacceptable level of inflation. But such explanations of the absence of the will to full employment in modern societies—even if unexceptionable as explanations—do not provide justification for eschewing full employment. For one thing, it is not clear that intelligent macroeconomic policy pursued by the state could not dampen the inflationary pressure of full employment. Secondly and more important, it is not clear why a substantial level of inflation should not be a morally tolerable price to pay in exchange for making decent employment available to the worst-off able-bodied members of society.

Is Full Employment Feasible?

Jon Elster (1988) has advanced an interesting argument against the feasibility of a legally guaranteed right to employment that the state would implement by assuming the role of employer-of-last-resort. The argument appeals to a plausible model developed by Shapiro and Stiglitz (1984) to explain why firms in a market economy would pay wages above what is needed to induce their employees not to quit their jobs (see also Carmichael 1985; Shapiro and Stiglitz 1985; Stiglitz and Weiss 1983). If a firm was paying its employees just marginally more than the wage that would render them indifferent between keeping their job and losing it, any worker who gets disutility from hard work would be motivated to shirk or loaf excessively on the job.[9] By assumption, the worker does not mind the prospect of losing the job (perhaps because it is easy to get another just as good). At least, this will be so if it is impossible or uneconomical for the firm to monitor the worker's job performance continuously, so as to prevent shirking. The firm solves this problem by paying its employees considerably more than their next-best employment, spot-checking their work performance, and threatening to give the sack to workers caught shirking. The employee's rational response to this policy announced by the firm is to refrain from shirking. If all firms adopt similar policies, the outcome will be (1) above-market-clearing wages paid to those who have jobs and (2) involuntary unemployment. In this situation, the threat of the sack involves the prospect of facing a spell of unemployment and hence induces employed workers not to shirk.

Elster uses this model to argue for the unfeasibility of state-guaranteed full employment. Suppose the state attempts to cure the unemployment problem by hiring, at wages comparable to those prevailing in the private sector, all those who wish to, and are qualified to work. In order to retain a credible threat of the sack for the purposes of reducing shirking, each firm will have to raise wages; but then a cycle ensues. If the state sector follows suit, raising wages to maintain comparability with the private sector, the private sector will also have to follow suit; but the private sector demand for labor falls and, if the process is replicated until all private firms are forced out of business, diminishes to zero. Elster draws this conclusion: "In capitalism, either some workers are unemployed or some workers are paid less than other workers doing the same job. Any attempt to escape this dilemma will have the same result: the state will be the sole employer, and all labor will be underemployed" (Elster 1988, 74). The last-quoted sentence alludes to the difficulty that in centrally planned socialist economies, state-managed enterprises have an incentive to hoard labor inefficiently, along with other resources that might be useful for the fulfillment of future state-imposed plan quotas. The result, according to Elster, is that labor under centralized planning regimes is "underutilized and degraded" (p. 72).

I want to avoid pedantic criticism of Elster's argument, which is deliberately stylized and illustrative rather than realistic in any detailed way. Does the argument point toward a deep problem with the proposal to achieve full employment by a legal right to employment? For several reasons I am doubtful.

Same Work, Same Pay?

I fail to see anything unattractive in one of the horns of the dilemma that Elster constructs. Suppose that the considerations that Shapiro and Stiglitz identify do force public sector wages to lag behind private sector wages for observationally indistinguishable job tasks and job performance. Why would this be so terrible?

If this minor inequity proved to be the necessary price of achieving full employment, I would say the price is morally acceptable.

Same work, same pay can hardly be a fundamental principle of justice in an economy that includes private firms, for the market can dictate different pay for same work. The Shapiro-Stiglitz model on which Elster attempts to rely is one of a family of models known as *efficiency wage theories* of the labor market (described in Akerlof and Yellen 1986, 1-21; Bulow and Summers 1986; Katz 1986; Lindbeck and Snower 1988, chaps. 1, 2; Sinclair 1987, 57-58; Stiglitz 1987; criticized in Kniesser and Goldsmith 1987, 1270-71). The general idea is that due to informational asymmetries, the quality of the labor performed for a firm may be a function of its price. Lowering the wage that a firm currently pays can increase labor turnover costs, attract a lower average quality of job applicants, cause the productivity of labor to decrease due to decreased employee morale, and in other ways cause firm profits to drop. A profit-maximizing firm will then set its wage offer so that at the margin, the increased labor costs from a small further rise would just equal the increased revenues due to the rise (this is the efficiency wage). One of the puzzling facts about labor market behavior that efficiency wage theories are attempting to explain is the observation of a stable distribution of wage offers for the same work.[10] An illustration is John Dunlop's study showing that in the Boston metropolitan area in 1951 unionized truck drivers were paid a wide spectrum of wages, with drivers in some industries paid almost double the wages paid to drivers in other industries. The same study showed many intermediate pay rates (Dunlop 1957; cited after Akerlof and Yellen 1986, 9-10). Such pay dispersion is common. The suggested explanation is that the efficiency wage in different industries may differ due to differing labor turnover costs or the like. But if pay dispersion is common in the private sector and passes unremarked, there is little reason to think that a pay gap between the public and private sector in the scenario that Elster describes would be invidious or cause despair among public sector workers.

Alternate Penalties for Shirking

For the sake of the argument I will set aside the point just made and assume that paying less for the same work in the public than in the private sector is deemed intolerable. Perhaps it is feared that over time the pay gap will lower the quality of the workers who offer themselves for public sector employment, causing deterioration of the sector. Or perhaps the pay gap is thought to be intolerably inequitable. We then face the other horn of the dilemma painted by Elster. Private firms are forced to raise their wages to maintain an edge above the public sector, so that they eventually go out of business—at which point the now-fully-public economy descends into the morass of centralized planning. I will not here challenge (though I do not accept) Elster's claim that a centrally planned economy must exhibit the distortions that we associate with Soviet-style economies. Even if this horn of the dilemma should prove sharp, it is easy to avoid, hence nonmenacing. The private firm wishes to find a policy that will give its employees adequate incentive not to shirk but cannot afford to raise wages in a full-employment economy with the state serving as employer-of-last-resort. The firm's problem is set by limits in the information it has available regarding its job applicants and limits in its ability to monitor the job performance of employees. So the employee who likes to shirk cannot be reliably screened out as an applicant and, once employed, will have opportunities to shirk. But notice that the firm does have some capacity to monitor job performance, otherwise the threat to sack those caught shirking would be idle. The firm, then, must have at its disposal alternate means of inducing employees to perform satisfactorily in the event that the combination of high wages and the threat of job loss proves unusable. For example, the firm can threaten workers caught shirking with a penalty rather than the sack. The penalty schedule and monitoring effort by the firm would be set so as to maximize its profit.

The objection might be made that in a full-employment economy, it will always be in the interest of the penalized worker to quit and immediately take another job rather than pay the penalty the firm wishes to impose: the penalties would be empty threats. The issue raised here seems important. One wonders whether an economy with anything like full employment would not offer too much security to the work force, which would carry too high a price in efficiency terms—just as an economy that guaranteed profits to all capitalist entrepreneurs would offer too much security to be viable.

The objection fails because it fails to take account of the reasonable adaptations to full employment that firms and individuals could devise. In a full-employment economy, firms (whether in the public or private sector) would have a greater incentive to investigate the quality and character of job applicants, since labor turnover costs are presumably higher when unemployed laborers are scarce. (However, this incentive might be counterbalanced by the circumstance that job-seeking workers are scarce in a full-employment economy, so firms would have an incentive to accept whatever applicants they can get.) A firm that inspects the prior work record of job applicants will be dissuaded from offering a good job to workers with dubious work records, other things being equal. A record of shirking creates a stigma. The more crucial the job to the firm's operations, the greater the incentive of the firm to check carefully and judge wisely. The reasonable and expectable responses of firms to a tight labor market will bring it about that the worker who spurns a penalty for shirking by quitting cannot expect instantly to acquire another job on comparably good terms. Thus, the threat associated with a schedule of penalties can be effective.

In fairness, a difficulty in the argument just given should be noted, though I do not think it a severe one. A firm that resorts to imposing penalties on employees caught shirking faces a moral hazard problem (Akerlof and Yellen 1986, 6). Having announced a penalty policy and posted a schedule of penalties for subpar work effort, it would be in the firm's interest to cheat in the course of enforcing the policy by imposing fines on workers who have not shirked. Once aware of this risk, workers would be averse to working at a firm that announced a policy of penalties for shirking. (This difficulty would not arise in the case of a firm that threatened to fire those caught shirking because the firm would gain nothing by firing nonshirkers.)

This moral hazard problem need not be insuperable. If unjustly fined workers complain more frequently than justly fined workers, a firm that cheats will tend to acquire a costly reputation as an unfair employer. The surest way for the firm to avoid acquiring a bad reputation is not to cheat. Morever, workers and the firm would have a mutual interest in setting up a procedure for monitoring the managers who administer the firm's antishirking policy. If loafing on the job is observable at all, it is observable by coworkers (and presumably by union representatives) as well as by managers, so the former can provide a check on the judgment calls of the latter. It sounds patently unrealistic to say, for most job settings, that when a worker is fired for poor performance, coworkers have no reliable way to judge whether the fired worker really was goofing off. Depending on the technology of the job and the organization of supervision, monitoring the disciplining managers may well be feasible even though observation of effort is costly. (But why should the workers trust their agents? There is another principal-agent problem here, which the employees would have to solve.) In a full-employment economy, there would be a strong incentive for employers and employees to work out a company policy of penalties for shirking that reduces the moral hazard problem to tolerable dimensions.[11]

Employer-of-Last-Resort or Sucker-of-Last-Resort?

Even if public sector enterprises are committed to a *Same work, same pay* ethic of wage parity with the private sector, that ethic surely does not require the public enterprise to offer the same pay for the same work to workers whose observable characteristics including prior work record mark them as likely to be less reliable or diligent than other workers in the same job slot. Equity does not require the public sector to be a *sucker*-of-last-resort. Moreover, we should distinguish the branch of the public sector that acts as employer-of-last-resort from the rest of the public sector. These last-resort public enterprises will be taking on unemployed persons desirous of work who have been either chronically or persistently out of work. The specter of unemployment looms very large over this class of persons, and the prospect of stable and decent employment is bound to appear a large benefit. There is no expectation here that the wage paid by the state as employer-of-last-resort should equal the terms of every applicant's last spell of employment—much less the terms that would fulfill every applicant's fondest wishes. For these reasons, the thought that full-employment policies must inevitably drive wages ever upward at all levels of the job hierarchy in the private sector strikes me as fanciful. The state committed to full employment is not thereby committed to offering every person who has lost a job—whether as janitor, engineer, or company president—comparable employment on equally desirable terms in the public sector. No egalitarianism would countenance any such policy.

Finally, the claim that a policy of guaranteed employment is unfeasible in an economy with a labor market that operates as the Shapiro-Stiglitz version of efficiency wage theory predicts obviously has no bite at all unless contemporary market economies really do exhibit that sort of labor market. But the empirical adequacy of efficiency wage theories of the labor market is a controversial matter.[12]

Must Job Creation Intended To Boost Self-Esteem Be Self-Defeating?

The suspicion arises that there is something inherently contradictory about the project of grafting a legal guarantee of employment opportunities onto a market economy. The operation of the market presupposes that adult citizens are responsible for securing their own livelihoods, whereas a legal right to work seems to express the idea that society should guarantee the livelihood of each citizen.

A somewhat less diffuse worry is that job creation to boost dignity and self-esteem is self-defeating. J. Donald Moon puts the argument in these terms:

> There is certainly scope for government action to create productive jobs that would not otherwise exist because of various institutional rigidities, or because they produce public goods that must be collectively provided. But if a job were genuinely productive, there would be no need to create it as part of a guaranteed jobs program. Because

> these programs provide work that is not productive, they violate the principle of reciprocity, and so fail to provide an adequate means for people to maintain their self-respect. (Moon 1988, 48)

Jon Elster advances a similar argument and adds that if state-guaranteed employment cannot provide self-esteem, it cannot provide other benefits either. If we set aside the pecuniary disadvantages of unemployment, which could be eliminated by cash transfers, we find that all significant nonpecuniary benefits derived from work depend on the work's being a source of self-esteem. (This is so because in the absence of self-esteem nothing else has much value, according to Elster.)[13] But the provision of nonproductive employment to the unemployed for the purpose of boosting their self-esteem is (Elster claims) necessarily self-defeating. Elster comments, "Self-esteem, like happiness, dignity, or innocence, belongs to the class of states that are essentially by-products" (Elster 1988, 74). The public provision of employment solely in order to boost the self-esteem of job recipients is alleged to be an absurdity, akin to an attempt to ease the pain of losing a race by passing out blue ribbon badges of victory to all contestants, including the ones who finished at the back of the pack.

Although Elster wishes to claim that boosting people's self-esteem by offering them make-work is conceptually impossible, he is also uncomfortably aware that it seems actually to happen. Therefore, he offers a hedged argument: subsidized (nonproductive) work intended to promote the self-esteem of job recipients cannot do so unless the subsidy is either (1) invisible or (2) justifiable on independent grounds that are generally known. The gist of his position is that provision of work whose sole rationale is to boost the self-esteem of the job recipient cannot attain its goal if the recipient is rational and knows what is going on.

The Elster–Moon arguments to the conclusion that job creation to boost dignity and self-esteem is conceptually impossible are flimsy. Two claims must be distinguished: one concerns *self-esteem,* the other *self-respect* (achieved dignity). My counterarguments against these claims are meant to prepare the way for a discussion of how job creation sensibly can benefit the lives of the disadvantaged.

Self-Esteem

Here the Elster–Moon claim is that in a competitive market economy, if people who suffer chronic or lengthy unemployment feel terrible about themselves, government provision of guaranteed jobs to them cannot alleviate their low self-esteem. This claim is doubly mistaken.

First, how one gets a job and what one does with the job once one has it are two different things. If you get a job by governmental largesse independent of any qualifications on your part except length of tenure in the status of unemployment, you would have to be a fool to take pride in the accomplishment. But a job is a bundle of opportunities, including the opportunity to do well or poorly at assigned tasks; and what the individual makes of those opportunities can readily attract the esteem of others and in this way enhance self-esteem. In order to serve as a vehicle for promoting the jobholder's self-esteem, the job itself need not be socially beneficial according to a reasonable cost–benefit analysis. But it must provide opportunities for a performance or achievement of some kind that attracts the admiration or esteem of others. A state-provided job might lead to association with coworkers who value a trait of mine that no other associates have found particularly noteworthy but in which I now gladly take pride. The knowledge that the voters of a certain political jurisdiction or a bureaucrat who set up the program provided the job just for the purpose of boosting the worker's self-esteem does not logically or causally block many sorts of processes by which self-esteem enhancement comes about.

Second, let us assume for the sake of the argument what I have just denied, that a job must be on balance socially productive in order to serve as a vehicle for reasonably enhancing the self-esteem of the jobholder. In fact (although it is obfuscatory to put the point in terms of alleged "conceptual necessity") I think there is something to the claim that state-guaranteed jobs that are make-work, such as raking the leaves off lawns that nobody minds seeing covered with leaves, will seem pointless to the worker and will therefore elicit lacklustre work effort and slipshod performance. The worker is not developing skills or work habits that will be useful in later regular employment and may be developing pseudo skills and bad habits that will have to be unlearned painfully in order to succeed at a real job. The worker may find cheer in fellowship with coworkers; but in the circumstances of make-work production such relations are unlikely to offer anything more than could be obtained from the street corner society of unemployed acquaintances. As Elster points out, the minimal benefit one will get from work is that a job structures one's life; but the structure imposed by make-work production may be dreary, so that even this rock-bottom benefit may be attenuated.

Even so, the appeal to the *willingness to pay* of individual consumers or of voting taxpayers as the test of whether a job is genuinely productive is off the mark. Willingness to pay is an obviously inadequate test of whether a job is productive so as to be a reasonable source of self-esteem. Suppose that the long-term unemployed are set to work (after training) providing day-care to the young children of working poor parents. Being poor, the parents cannot afford the child-care services. Let us further suppose that the voters—and their political representatives who designed the program—are age-biased and thus were motivated solely by concern for the unemployed, rather than any concern for the children. Still, the children are obviously thriving in this program and visibly grateful to the child-care providers. The clear social benefits of the work the child-care providers do can be a source of self-esteem. Or suppose that the long-term unemployed are set to work on projects that will greatly benefit people who will live a hundred years from now. Toxic wastes have been placed in the ground in containers that will leak many years from now unless repaired, and the repairs are many times less costly and less dangerous if done now. The long-term unemployed who work on these repairs may be the only persons alive today who fully appreciate the long-run utility of their task; still, their sense that what they are doing is worthwhile can be a reasonable source of pride and self-esteem.

Self-Respect (Dignity)

In the Elster-Moon argument *self-respect*, or *dignity*, means an agent's sense of dignity that rises or falls depending on whether or not the agent judges that he or she is fulfilling moral standards "that define what it is to be a person of worth, a person entitled to respect" (Moon 1988, 32). The individual does not have complete freedom to select these standards; for the most part, they are derived from society.

In a society organized around a market economy, it is argued, a prerequisite for full membership in society is maintaining independence by being self-supporting. To keep one's self-respect one must satisfy this independence standard, which "requires at least able, working-age men to provide for their own needs and those of their family" (Moon 1988, 33). An idiosyncratic individual might uphold standards of personal dignity that have nothing to do with success on the market, but ordinary persons will accept prevailing standards as a matter of course. It follows immediately that an able-bodied person who gets a living by accepting transfer payments from the state forfeits dignity. Similarly, if society offers a subsidized job to an unemployed disadvantaged person out of charity, this is a disguised transfer payment: here, too, the recipient of charity loses *dignity*. This will be so even if the job experiences help the person to feel good about him- or herself and gain enhanced *self-esteem* in other ways.

It is doubtful that the somewhat vague empirical claim that this argument asserts regarding the winning and losing of dignity is correct. As an issue in the sociology of moral consciousness, there is good reason to doubt that being self-supporting is either a *necessary* or a *sufficient* condition for retaining dignity. (Compare the skeptical discussion as to whether invocation of the "work ethic" can help explain people's work-related behavior in Kelvin and Jarrett 1985, 100–194.) That self-support is not a necessary condition of dignity is proved by the observation that agricultural price supports are an uncontroversial policy in many democratic market societies. It is fanciful to suppose that farmers are so fervently attached to free-market norms that they lose their dignity when they receive the government subsidies they so assiduously seek. That it is not a sufficient condition is proved by the fact that faith-healing television evangelists, lawyers whose clients are gangsters, and producers of gaudy children's toys that fall apart shortly after purchase may be spectacularly self-supporting yet not spectacularly self-respecting.

But even if it were correct that in contemporary society being self-supporting is a prerequisite to maintaining one's dignity, it is puzzling that Moon thinks this would signal a serious contradiction in the idea of a "welfare state" committed to redistribution to raise the life prospects of the losers in free-market competition. If, as Moon supposes, people ordinarily draw the standards they live by from their social context of institutions and practices, then when living in a reformed society in which prevailing institutions were shaped by the welfare state, people would come to bear allegiance to welfare state standards and judge citizens' conduct accordingly.

More important, Moon suggests no reasons to support the independence norm. Baldly stated, it hardly sounds attractive. Thus, if proposed (mildly) egalitarian redistributive policies would conflict with it, this still fails to condemn those policies.

Suppose that an egalitarian society organizes redistributive programs that supplement the incomes of the working poor (e.g., along lines suggested in Ellwood 1988, chap. 4). The people aided by these programs are working at tough jobs for low pay. They are, let us say, doing the best they can. No doubt they would be happier and feel better about themselves if they could secure better jobs. But I see no reason to suppose that acceptance of transfer payments, which would violate the independence norm, ought to reduce or destroy their dignity. After all, we cannot sensibly be held responsible for what lies beyond our control; and clearly, many factors strongly affecting our ability and opportunity to be self-supporting are beyond our control. The most stringent version of the work ethic that could make sense if one accepted this point would assert that we ought to make reasonable good faith efforts to be self-supporting. Working part-time, sporadically, or in jobs artificially insulated from some market pressures by state subsidy is the most that some emotionally disturbed or low-skill persons could manage. The rugged individualist ideology that says it is demeaning for such persons to accept charity from the state is not, I submit, a doctrine that egalitarians should attempt to splice into their principles. If they do, they face the "contradictions of the welfare state" that worry Moon and Elster. But the problem here is entirely an artifact of consorting with an enemy that should rather be fought tooth and nail—even if it *is* presently part of the "self-understanding" of many citizens of contemporary societies, who find in the practices of a romantically imagined unregulated market economy "ideals and principles that give meaning and point to their lives" (Moon 1988, 28).

Decent Employment Opportunities

If the state offers a legal guarantee of employment opportunities to those who suffer chronic and persistent unemployment, what sort of opportunities should this guarantee promise? I think the considerations that trouble Elster and Moon point toward at least the beginning of answers to this large question.[14]

Abstractly, one can note that the jobs provided by the state in its role as employer-of-last-resort should be made as attractive as possible for their target recipients, up to the point at which any further sweetening of the last-resort jobs would hurt, rather than help, the disadvantaged. To abbreviate what should be a long discussion, I tentatively suggest that if last-resort state sector jobs are to function to boost the self-esteem and dignity of their recipients significantly, they should satisfy nine guidelines.

1. *No Make-work.* The jobs provided by the state must be manifestly socially beneficial. In Boston during the 1890s a member of the Boston Citizens' Relief Committee described the nonproductivity of the relief work the committee offered to the unemployed: "We had to employ them in some work that was from an economical sense useless. We could not employ them in making things that would interfere with other people, and that is why we adopted the absurd alternative in one respect of making rag carpets because nobody wants rag carpets and therefore we could make them without interfering with anybody's industry" (quoted in Keyssar 1986, 255).

2. *Stable Employment.* The state-guaranteed jobs are not conceived as a temporary stopgap. An individual who performs satisfactorily at a last-resort job should have the security of continued employment.

3. *Some Promotion Possibilities.* There should be a ladder of advancement.

4. *Low Skill Requirements.* If the state as employer-of-last-resort designs jobs that require a high skill level, the truly disadvantaged are unlikely to derive much benefit from the scheme.

5. *Careful Monitoring of Task Performance—No Perverse Incentives.* A job cannot be a vehicle for building self-esteem if no one minds whether the job is done well or poorly. Work rules, promotion procedures, and supervision should be organized so that the better the individual's job performance, the greater the benefits derived from the job. It would be cruel to impose minimum performance standards that would make last-resort employment a revolving door for those who need it most. But there should be consistently enforced, if gradually introduced, standards; no positive incentives for malfeasance; and after a probation period, a serious judgment concerning tenure.

6. *Respectful Supervision.* Freedom from arbitrary and demeaning supervision would include the possibility of transfer to activities that offer increasing freedom from close monitoring. But even watchful supervision—surely necessary for entrants—should be carried out in a respectful and noncapricious fashion. Two obvious means to this goal are to make supervisory jobs sufficiently desirable to attract qualified applicants and to promote collective organization of the workers being supervised.

7. *On-the-Job Training.* Where possible, on-the-job training should emphasize the development of generally useful and nonspecific skills that will help to give last-resort workers the option of rejoining the private sector (or the regular public sector) of the economy.

8. *Genuine Choices.* The state should provide a choice among genuine alternate entry-level last-resort positions.

9. *Lesser Eligibility.* Last-resort employment should offer net benefits that (1) exceed what one whose status shifted to the non-able-bodied or nonemployable category could get in transfer payments and (2) fall short of the worst-available private sector employment (which the state does not wish to drive out of existence for egalitarian reasons).

Reasonably generous employment practices in the last-resort sector will exert a gravitational pull, drawing workers from the worst jobs in the private and regular public sectors. In effect, the state is creating a demand for chronic and persistent unemployment, since this is the eligibility condition for entry into the last-resort jobs. To some extent this is an ineliminable cost of such a policy; within limits, it is surely a tolerable cost.

If the state organizes a last-resort employment sector with jobs that are more attractive than the currently worst low-skill jobs in the private and regular public sector, firms are given an incentive to sweeten the terms of their worst low-skill jobs and to redesign their production process so that more of their work can be done by high-skill labor, since marginal low-skill labor is now rendered more expensive. The end result should be an overall improvement in the terms of work offered to low-skill disadvantaged workers prone to unemployment and sporadic employment.

But this end result would not be utopia. It is impossible to devise policies that discriminate perfectly between the able-bodied and non-able-bodied and between the disadvantaged unemployed and the nonneedy bohemians. In the ideal feasible egalitarian society some who deserve state aid would not get it. Similarly, some who do not deserve aid would get it: a decent society would support at state expense some of the loungers and scroungers who so irked Spencer.

Notes

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1. Egalitarian welfarism and maximin welfarism yield very different implications for policy, so this simplification is consequential. I reject maximin welfarism for reasons noted in n. 4.

2. A shift from state-of-the-world A to state B that is on balance socially beneficial need not be a Pareto improvement. But if A is Pareto-inferior to B, the move from A to B must be on balance socially beneficial.

3. See the discussion in Rawls 1971, chap. 5. (The principle that Rawls envisages implementing is concerned with the distribution of primary goods, not welfare.)

4. How exactly is the worst-off class (in welfare, primary goods, whatever) to be defined? The notion of the worst-off class is fuzzy in a way that conceals a problem. Given a clear standard for being better or worse off, there will, except in the case of ties, be a single individual who is worse off according to that standard. There is no theoretical rationale for broadening the scope of the worst-off class to include a range of persons many of whom are better off than the worst-off. At least, maximin advocates like Rawls give no convincing rationale for broadening the concept. One suspects that the maximin advocate speaks of the worst-off class as a class containing numerous members to avoid the implication that a maximin policy that gives strict lexical priority to the interests of the worst-off could recommend a policy that gives a tiny gain to a single person, the worst-off, at a cost of causing significant losses to many persons who are very badly off. See Rawls 1971, 95–100, esp. 98.

5. This is simple-minded for at least two reasons: (1) there are many different types of ability so it is misleading to speak of "ability" *simpliciter;* and (2) real-world market economy will tend to reward not just ability but also personal characteristics (such as

being white-skinned and male) that are favored by the prejudices of those interacting on the market.

6. It should be noted that a cash grant can make it feasible for a person to accept a job that offers attractive nonpecuniary satisfactions but insufficient pay to ensure a tolerable standard of living. In this sense a person can purchase employment by means of a cash grant.

7. *Leisure* may be a misleading term for time spent off the job. Some of this time may be spent in drudgery to satisfy needs. For example, instead of working for pay for an extra hour per week and eating in a restaurant, I might choose to grow vegetables and cook my meals at home. This choice is not accurately described as one of work versus leisure.

8. *Self-esteem* here refers to what John Rawls calls "self-respect" (1971, 440).

9. *Shirking* is defined as putting forth less work effort than the company norm. If this norm is efficiently set, shirking behavior will be Pareto-inefficient. If workers greatly enjoy on-the-job leisure, an efficient contract between workers and their employer will set the effort norm so that on-the-job leisure that is worth more to the worker than it costs his employer will not be in violation of the norm.

10. There is some slippage here in the notion of "observationally indistinguishable employees doing the same work." If the efficiency wage differs between firms or between industries, there is some empirical difference in the kind of work being done. Suppose two identical workers are pushing identical brooms in exactly the same manner. But one works for a firm with a higher capital to labor ratio, and if his broom injures the adjacent machinery, the costs to the firm are higher than if the counterpart worker does "the same" to the cheaper machinery. The firm that is more vulnerable to carelessness may find it profitable to pay a higher wage to induce more careful work.

11. A complication worth exploring is that workers who are highly reliable might have an above-average aversion to close supervision. A firm that announces a policy of heightened spying on employees to curtail shirking might thereby cause the quit rate of its highly reliable employees to rise and its pool of job applicants to contain fewer highly reliable workers than would otherwise be the case. For related discussion, see Akerlof 1982, 1984; Burawoy 1979; Kohn 1976.

12. In theory, firms would not need to pay wages above the market-clearing level if they could require newly hired workers to post a sufficiently large performance bond that would be forfeited if the worker was caught shirking and dismissed. To avoid the moral hazard difficulty that the firm would then have an incentive falsely to claim employee malfeasance in order to appropriate the bond, either the bond could be made payable to some third party about whose welfare both the workers and the firm are indifferent or else reliance could be placed on the firm's desire to maintain a reputation as an honest employer (see Carmichael 1985; Shapiro and Stiglitz 1985). In practice, firms do not require such performance bonds. It has been claimed that upward-sloping earnings profiles can substitute for performance bonds. The worker who shirks and is dismissed or is simply passed over for promotion loses the expected value of the wage gains contingent on steady exemplary work performance. It is helpful to think of these wage gains as given to the worker in the form of a trust fund to be made available upon retirement. This type of arrangement will not suffice to deter a utility-maximizing worker from shirking early in his or her career so long as the expected gain from shirking exceeds the expected gain from working hard, as it must if the value of the trust fund set aside for the worker is small at first and grows gradually; that is to say, upward-sloping wage profiles are not a perfect substitute for performance bonds—at least, they are not if the worker caught shirking early in his or her career can costlessly transfer to a next-best employment opportunity that will be virtually as good as the present job (as it will if the labor market is fully competitive). However, if a stigma attaches to dismissal owing to shirking such that the worker caught shirking even early in his or her career faces a worse set of job opportunities in consequence, it may be possible for firms adequately to motivate their workers not to shirk without paying them above-market-clearing wages (see Akerlof and Katz 1989).

13. I doubt that this is correct. Low self-esteem can be a spur to accomplishment, which boosts the degree to which a person fulfills his or her most important goals (attains welfare). Even at very low self-esteem levels that inhibit significant effort directed toward goals, does a beer or an ice cream cone taste less good? Is love less appreciated? Even if there is a minimal self-esteem level for each person, below which nothing in life has much value, there is no reason to expect these individual thresholds to lie above the self-esteem level to which unemployment pushes each person.

14. A useful starting point for reflection on this topic is dual labor market theory, which maintains that "jobs can be roughly divided into two groups: those with low wages, bad working conditions, unstable employment, and little opportunity for advancement (secondary jobs), and those with relatively high wages, good working conditions and opportunities for advancement." See Dickens and Lang 1985, 792; Doeringer and Piore 1971. See also Bulow and Summers 1986.

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THE COMMITTEE ASSIGNMENT PROCESS AND THE CONDITIONAL NATURE OF COMMITTEE BIAS

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The view that congressional committees tend to be biased subsets of their parent chambers provides the foundations for a substantial body of theoretical literature on distributive politics and legislative structure. More recent revisionist work suggests that committees composed of preference outliers are in fact rare. We reject the categorical account of preference outliers a priori *and elaborate conditions under which committees should be unrepresentative of their parent chambers. We argue that the most widely available and frequently used data—floor roll call votes—are inappropriate to the task of assessing outlier predictions in any form. Finally, we conduct a differentiated set of hypothesis tests within one policy jurisdiction to illustrate the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization. The appearance of policy-relevant biases in congressional work groups, we conclude, is not so much rare as it is conditional, and we suggest several conditions on which future models of legislative organization should build.*

Students of Congress have long held that the organizational structure of the institution is not politically neutral, that the committee system in particular produces systematic biases in congressional policy making. Within certain important limits, members have considerable influence in selecting the committees on which they serve, so that, over time, many committees are populated with legislators who have strong interests in matters within their work groups' jurisdictions and presumably strong attachments to the programs that these groups oversee. This account has attained the status in some quarters of a stylized fact and provides the foundations for a substantial body of theoretical literature on distributive politics and legislative structure (e.g., Shepsle and Weingast 1987; Weingast and Marshall 1987), as well as periodic calls for democratic reform (e.g., Ornstein 1974; Rieselbach 1986). More recent and more comprehensive empirical work suggests, however, that this account may be a stylized *fiction*—that committees composed of preference outliers are in fact rare (Krehbiel 1990, 159). To the extent that this revisionist view is correct, its implications are central to our understanding of decision making and political representation in what is, ostensibly, a majority-rule institution.

We contend that both the stylized account of committees as preference outliers and the revisionist view—that preference outliers are rare—mischaracterize the reality of legislative organization. Extending the literature on committee and subcommittee assignment processes, we establish the theoretical foundations for

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rejecting the categorical account of preference outliers *a priori* and elaborate a more differentiated set of conditions under which we should predict committees to be unrepresentative of their parent chambers. Second, we argue that the most widely available and frequently used data—floor roll call votes—are inappropriate to the task of assessing outlier predictions in whatever form, demonstrating how problems in measuring preferences conspire to produce null results. Finally, we conduct a specific and differentiated set of hypothesis tests within one policy jurisdiction, thereby illustrating the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization. The appearance of policy-relevant biases in congressional work groups, we conclude, is not so much rare as it is conditional; and our analysis suggests several conditions on which future models of legislative organization should build.

The Conditional Nature of Committee Bias: Theoretical Foundations

Are committees composed of preference outliers? In addressing this question two possible tacks are available. One tack is to formulate the outlier hypothesis as a more or less categorical claim and test it for all committees. In some respects, this is not an unreasonable point of departure. Stylized accounts of committees have taken a central place in formal theories of legislatures; and there is some tendency among empirical scholars, in moments of summary description, to overstate the evidence of committee bias. In a recent and important paper, Krehbiel (1990) thus subjects the categorical claim to a systematic examination and finds it seriously wanting. In so doing, he provides a necessary corrective to unwarranted simplifications in both formal and empirical research.

Several arguments sugest that a more differentiated set of outlier hypotheses is important, however, if we are to move beyond the enterprise of accepting or rejecting stylized claims and develop a satisfactory theory of legislative organization.[1] First, is Fenno's introductory postulate on the study of committees, committees differ, and the implication he derives from it, that political scientists' search for explanation is subverted by "empirical generalizations . . . uttered as if [they] were equally applicable to all committees" (1973, xiii–xiv). Second is the empirical literature on committee bias that questions whether the stylized fact commonly employed in formal models of legislatures approximates what legislative scholars actually claim. Most statements of the outlier view only suggest that "*many* committees are biased . . . *in one way or another*" (Davidson 1981, 111, emphasis ours). Third and most importantly, the literature on committee and subcommittee assignment processes provides theoretical foundations for rejecting the categorical claims a priori in favor of more precise and more clearly justified conditional claims. While this literature does not provide a fully developed condition-rich theory of preference outliers, it does (to paraphrase Fiorina) provide theoretical guidance regarding precisely what we expect to find and where we expect to find it (1974, 25). It thus permits us to push the empirical study of this important question beyond Krehbiel's useful beginnings.

We extend here the most sophisticated work in this area, Kenneth Shepsle's *Giant Jigsaw Puzzle* (1978; see also Rohde and Shepsle 1973). Three important components to Shepsle's theory bear reviewing here. First, committees are "*jurisdiction-specific* subunits of their parent chamber" (Shepsle 1978, 4; emphasis in original). Second, the specific nature of the "jurisdictional distinctions" leads members with different interests to evaluate the benefits of different assignments differently (pp. 4–5, 45). These subjective evaluations drive members' seeking of assignments and, given the tendency of the party committees on committees to accommodate member requests, produce a process that can be characterized, in the main, as self-selective. As Shepsle summarizes, "It is the jurisdictional characteristics which attract the distinctive membership" (p. 5). Third, Shepsle's is a theory of "*constrained* maximization in an institutional setting" (p. 7; emphasis ours). Self-selective efforts on the part of assignment seekers and accommodationist tendencies on the part of party leaders occur in the context of four significant procedural constraints: (1) House members are typically limited to no more than two major committee assignments; (2) while committee sizes are adjustable at the margin, the number of seats on each committee is limited by the majority party to some relatively small proportion of the House membership; (3) the committees on committees place limits on the number of representatives from a single state or zone that can sit on a particular committee; (4) most importantly, a long-standing House rule regarding party representation requires that with few exceptions, committees exhibit party ratios in rough proportion to the ratio evident in the parent chamber.

What, then, does committee assignment theory suggest in the way of hypotheses about preference outliers? First and most generally, nothing in this theory gives rise to the view that self-selection operates automatically and uniformly, notwithstanding the popularity of stylized accounts about homogeneous preference outliers. Second and more specifically, there is no theoretical reason to expect that self-selection will produce a policy bias in committees that lack a distinctive policy jurisdiction, such as House Administration, Rules, or Standards of Official Conduct. Tests for such committees are thus tests of predictions that committee assignment theory does not make. Third, Shepsle's theory implies that a self-selection-generated bias will be contingent on the breadth and diversity of a committee's jurisdiction. There are two reasons for this prediction. First, committees with broad, diverse jurisdictions necessarily evoke the interests of large numbers of members, leading to a substantial competition for available slots. Interest-driven self-selection thus plays a much smaller role in the determination of committee composition not because of any institutional unwillingness to accommodate member requests but because of the constraints imposed by the assignment preferences of other interested members. Throughout Shepsle's discussion of the assignment process, slot availability and competition are important and recurrent themes; and he suggests that they matter most for understanding assignment to exclusive, or otherwise broad-scope committees. The second reason for this prediction is simply that the more diverse a committee's jurisdiction, the less it can be characterized as the policy-specific unit that might generate an identifiable bias of interests. If a small subset of members seek assignment to House Energy and Commerce largely because of their district's interest in foreign trade, another subset because of their interest in clean air, and still others because of their interests in telecommunications policy, natural gas pricing, or health care, then the probability that the full committee will appear biased along any one of those dimensions is diminished.

Of course, how one discriminates between broad, heterogeneous jurisdictions and more narrow, homogeneous ones is not obvious; and Shepsle provides no systematic method for doing so (for one attempt, see Hinckley 1975). But while fine distinctions are not easy, several House committees clearly fall into the broad scope category: Appropriations, Ways and Means, Budget, and Energy and Commerce. Indeed, Krehbiel (1990) takes note of Davidson's (1981) observation that the first three of these are exceptions to the outlier account; and various observers have emphasized the "octopus-like quality" of Commerce's jurisdiction (Shepsle 1978, 88; see also Stanfield 1988). Hence, if these committees fail to confirm the outlier hypothesis (as all of them do, according to Krehbiel's first results), they can hardly be taken as evidence that the theoretical account of self-selection is incorrect, though they do serve to remind us that stylized renderings of that account are, in fact, stylized. A theoretically more defensible formulation of the outlier hypothesis, on the other hand, replaces categorical with conditional claims: the second condition deducible from assignment theory is simply that a committee-level bias will depend on the jurisdictional heterogeneity of the committee.

This is not to say, however, that one can necessarily dismiss the matter of work group bias for committees that enjoy a policy jurisdiction broad in scope. When committee jurisdictions are diverse enough to attract different members pursuing different sorts of interests, the committee may differentiate itself in such a way that interested subsets of the committee form, and enjoy disproportionate influence over more discrete parts of the larger committee jurisdiction. Indeed, in the last two decades this has increasingly happened through the creation and institutionalization of subcommittees (Haeberle 1978), especially in the House (Smith and Deering 1984). Moreover, the subcommittee assignment process is governed by self-selection far less constrained than in the committee assignment process. Given that within-subcommittee seniority is not the basis for ascension to subcommittee leadership positions, members are not limited by the expected costs of queue switching and subcommittee sizes are far more easily adjusted to bring the supply of seats in line with member demand (Hall 1990). Within committees, in turn, subcommittees tend to exert disproportionate influence over matters within their jurisdictions (Hall and Evans 1990).[2] In our view, then, a theoretically more plausible statement of the outlier account suggests a third condition: the appearance of a policy-relevant bias in jurisdictionally diverse committees is more apt to occur at subcommittee level than at full committee level.[3]

The case for focusing on subcommittees thus turns on the premise that one's analysis should be tailored to theoretically significant features of the legislative organization. But more generally, the committee–subcommittee distinction raises the matter of the level at which the analysis of bias is appropriately pitched. If, as Smith (1988) points out, significant policy biases vary with the issue, theoretical statements about work group bias that do not specify the issues for which the statement holds are as apt to mislead as to inform. Committee (or subcommittee) jurisdictions that include qualitatively different issues may be biased in predictable but different directions depending on the issue. Consider the case of the food stamps program in the House Agriculture Committee. Relative to the rest of the committee's jurisdiction, food stamps is the odd program out—a redistributive program for the urban poor in a committee "dominated by members with a primary interest in serving their rural constituents" (Ferejohn 1983, 137). As a consequence, one would not expect that committee members' preferences would be inherently more favorable toward the program than the parent chamber. To the extent that members' preferences regarding federal nutrition programs are based on constituency demand, in fact, one would be more likely to predict the reverse. This is not simply an ad hoc observation; the general principle at work in producing this hypothesis derives directly from the theory of self-selection, which posits that committees have "distinctive" jurisdictions. Programs that are relatively unrelated to the major or salient dimensions of the committee's jurisdiction (as perceived by interested assignment seekers) predictably will not enjoy a bias in their favor; indeed, findings of such null results should be counted as exceptions that prove the self-selection rule. If one is to assess the outlier hypothesis properly, then, an issue-specific analysis is almost certainly preferable and, on theoretical grounds, often necessary. Even (committee or subcommittee) jurisdiction-specific indicators, in other words, may mask significant biases in committee preferences with respect to specific but important issues, bills, or subjurisdictions.

To summarize, we strongly agree with Krehbiel's (1990) recent claim that the stylized account of preference outliers mischaracterizes the reality of congressional organization. But we also suggest that the study of committee bias must move beyond the testing of stylized claims in favor of a more differentiated and theoretically justified set of outlier hypotheses. To Fenno's admonition about committee-level generalizations, we would simply add that hypotheses uttered as if they were equally applicable to all subcommittees and all issues may also tend to mislead. In the next two sections, we shall show how attention to such distinctions directly affects the substantive conclusions one is likely to reach about committee bias.

Problems of Data and Method in Testing Outlier Claims

The enterprise of testing alternative theoretical claims about committee bias is plagued by a number of complications. The unobservability of preferences requires that the researcher confront complicated inferential problems when using behavioral data, and the development of appropriate statistical tests has received remarkably little attention. On both counts, Krehbiel's (1990) analysis of preference outliers provides important advances over existing work on the subject. We find especially important his argument that we distinguish between interest intensity and preference homogeneity. As Krehbiel points out, there are at least two plausible conceptions of committees as outliers. In one, the Niskanen (1971) model, committees are homogeneous, composed of those with relatively high demand for programs to serve their particularized needs; while in the other, we might merely expect to see those with relatively intense interests in an issue represented on the relevant committee, with no clear expectation that these interests would be mutually compatible. By elaborating the intensity-homogeneity distinction, Krehbiel provides an important guide to the empirical study of policy-relevant committee bias.

Important problems of data and method remain, however, and these problems conspire to produce no-difference conclusions. Our principal concerns follow directly from the use of roll call voting data to test differences in committee-chamber preferences. The first and most damaging problem is that not only are roll call votes error-filled measures of member preferences (Jackson and Kingdon 1990), but the measurement error is systematically related to the principal variable of interest—committee membership. Reconsider the outlier account in its most stylized form: (1) committee jurisdictions are sufficiently differentiated to permit a clear matching of member interests to assignment portfolios; and (2) members select their own committee assignments, so that committee members are significantly more likely to support programs within their jurisdictions and more likely to hold those preferences with greater intensity. Under such a scenario the conditions for logrolling between committee members and nonmembers are ripe. Members sacrifice their weakly held preferences on matters outside their own committees' jurisdictions, taking their cues from relatively intense committee colleagues, in exchange for reciprocal treatment on matters before their own panels or other favors that may not involve votes (e.g., holding a field hearing, cosponsoring a bill, helping with a campaign). Indeed, such bargaining is often cited as an important means by which committees and other intense preference minorities build majority coalitions (e.g., Jackson 1974; Kingdon 1981, 100–101; Matthews 1973), despite the fact that incentives to defect from prearranged bargains render such coalitions somewhat fragile (e.g., Ferejohn 1983; Weingast and Marshall 1987). In addition, even where no implicit or explicit exchange operates, nonmember scrutiny of committee proposals entails time, staff, and information costs—costs that are less likely to be incurred by nonmembers expecting only limited benefits from pursuing preferences on what for them are relatively low salience issues. "Going along" on the floor, then, is not simply a norm that one abides in order to "get along"; it is the consequence of rational calculations by committee nonmembers about how to best spend their legislative time (Hall 1990).

In short, if one imagines a classical scenario under which committees are biased and powerful (i.e., committee members' preferences tend to be extreme and intense and their preferences are deferred to by nonmembers), we would expect to find nonmember voting patterns that mirror committee member patterns—a no-difference result! What Krehbiel (1990) cites as evidence that the stylized outlier account is wrong, in other words, is consistent with a stylized view that is right. For present purposes, however, one need not accept that intercommittee reciprocity is widespread (nor that existing theory adequately explains how the incentives to renege are overcome), only that its occurrence is nontrivial: to the extent that it occurs at all, roll call data will understate committee-chamber differences.

Even were roll call data appropriate to the task of measuring preferences, the use of interest group-generated indexes further inflates the likelihood of a no-difference finding. Such indexes are simply not well tailored to the jurisdiction-specific hypotheses being tested. This is inherently true of Americans for Democratic Action (ADA) scores. As we discuss above, an important foundation of the theory of self-selection is that committees are jurisdiction-specific units; it is the nature of a committee's jurisdiction that makes it more or less appealing to particular subsets of members. Committee assignment theory, therefore, does not lead one to predict that committees will tend to be biased along some scale that is not jurisdiction-specific—that in fact purposely collapses policy domains into a single dimension. Krehbiel's ADA-based analysis of committee-chamber differences for the House's 22 standing committees thus lacks grounding in the theoretical literature that gives rise to outlier predictions in the first place, a problem that cannot be addressed by recommending "extreme caution" in interpreting the results (Krehbiel 1990, 155).

While more defensible on theoretical grounds, more specialized group ratings are subject to similar validity problems. Even the best of these ratings are still not jurisdiction-specific. For example, the National Farmers Union (NFU) rating, which according to Krehbiel provides the "most jurisdiction-specific information of all ratings" (1990, 157), typically includes votes on topics that extend well beyond the jurisdictional boundaries of the agriculture committees, such as passage of omnibus appropriations bills, balanced

Table 1. Committees As Voting Outliers Revisited: The Case of the Senate Committee on Agriculture, Nutrition, and Forestry, 99th Congress

Voting Index	Committee Median	Chamber Median	Difference in Medians	Committee Member Mean	Committee Nonmember Mean	Difference in Means
Americans for Democratic Action Index	20	45	-25	33.9	44.3	-10.4
National Farmers Union Index	58	58	0	49.8	52.9	-3.1
National Farmers Union Index "Corrected"	75	54	21	53.9	53.7	.2

Sources: Michael Barone and Grant Ujifusa, *The Almanac of American Politics 1988* (Washington: National Journal, 1987); "How Special Interest Groups Rated Senators," *CQ Weekly Report*, 22 November 1986 and 22 August 1987; "1985 Voting Record" 1986a; "1986 Voting Record" 1986b.

budget amendments, MX missile development, and social security financing (see "1981 Voting Record" 1981; "1985 Voting Record" 1986a; "1986 Voting Record" (1986b). Likewise, the AFL-CIO's Committee on Political Education score typically includes votes on such matters as federal food stamp benefits, natural gas decontrol, and voting rights—none of which fall under the jurisdiction of either the House or Senate labor committees (see e.g., Kirkland 1986a, 1986b). To the extent that these ostensibly policy-specific indexes are based on issues unrelated to the relevant committee's jurisdiction, they introduce measurement error that will systematically diminish the likelihood that a statistical test will establish a committee–chamber difference when one exists.

Table 1 illustrates this point. In order to show how even the most jurisdiction-specific ratings can bias the results in favor of the null hypothesis, we revisited the case of the Senate Agriculture Committee in the Ninety-ninth Congress. In addition to replicating Krehbiel's analysis for ADA and NFU scores, we created a "corrected" NFU score eliminating all votes on which the NFU rating was based that clearly did not deal with agriculture, nutrition, or forestry issues—fully one-third of the votes NFU selected for the two-year period.[4] The comparisons of committee and chamber means and medians are reported in Table 1. As a quick review of the table reveals, the committee–chamber comparisons look very different depending on whether the index is constructed to be jurisdiction-specific. Where the index includes votes that have little specific relevance to Agriculture's jurisdiction, one is led to infer that the committee is conservative with respect to agricultural programs, exhibiting a difference in median ADA scores of −25. When the index is based on votes drawn heavily but not exclusively from agricultural issues (the score that NFU reports), the committee and chamber medians are identical. Where the index is restricted solely to agriculture votes, however, the difference in medians jumps to 21, dramatically reversing Krehbiel's tentative inference that Agriculture is, if anything, more antiagriculture than the Senate as a whole (1990, 158). The pattern for the difference-in-means test is less striking, with the observed difference changing from clearly negative, using ADA scores, to slightly positive, using the "corrected" NFU score; but the results for both medians and means clearly demonstrate the no-difference bias inherent in indicators that are contaminated by votes unrelated (or only weakly related) to the relevant committee's jurisdiction.

We conclude this section with a comment about the standards of demonstration appropriate to evaluating alternative theoretical claims about committee bias. At the core of Krehbiel's argument is a set of hypothesis tests that are ostensibly simple and convincing. In particular, he sets up a committee–chamber difference-in-means test, employs the conventional .05 significance level, and finds that the null hypothesis can be rejected in exceedingly few cases, regardless of the year or chamber being studied. Such findings, in turn, lead him to make the substantive claim that biased committees are rare (1990, 159) and to suggest that such findings provide encouragement to incomplete-information theories of legislative organization, which predict that Congress will prefer unbiased committees because of their greater willingness to share policy expertise (p. 160).

Based on Krehbiel's own results, neither point is justified. The problem derives from the fact that given his substantive thesis, Krehbiel minimizes the wrong type of statistical error. As Julnes and Mohr summarize the long recognized but often forgotten principle, "Whereas traditional conclusions of 'difference' seek to minimize Type I errors, no-difference findings need to minimize Type II errors (i.e., failing to reject the hypothesis of no difference . . . when it is in fact false" (1989, 629; see also Blalock 1979, 159–65). By setting the significance level at the conventional .05, in other words, Krehbiel unintentionally biases the interpretation of his results in favor of his principal argument and, by implication, an alternative theoretical perspective on legislative organization. Where one wishes to make a no-difference claim (more accurately, an "approximately-no-difference" claim), as Krehbiel does, the researcher must use a considerably larger alpha level; Julnes and Mohr recommend .25, one-tailed (1989, 641–44; see also Blalock 1979, 161). Had he used this more appropriate standard (t-statistic $\geqslant .68$), Krehbiel's conclusions would have appeared very different: despite the no-difference bias associated with the voting indexes, seven of ten committees pass the threshold for at least one of the relevant "policy-specific" ratings that he employs. Hence, Krehbiel's conclusion that "few contemporary committees are composed predominantly of high-demand preference outliers" (1990, 159) is simply not supported by his results. More generally, the evaluation of theories of legislative organization that predict representative committees will require a different set of standards and a more complicated set of tests than those supporting conventional outlier predictions. Likewise, theories that suggest more conditional claims, including, say, hypotheses of difference for some committees and some issues and no-difference hypotheses for others, will require statistical standards that vary, test by test.

In summary, interest group indexes are not simply imperfect indicators, they suffer from measurement error that systematically understates differences between committee and chamber. More generally, they reflect the no-difference bias associated with any measure of preferences based on roll call data, thereby inflating the probability of retaining the null hypothesis when it is in fact false. Finally, Krehbiel's misapplication of conventional significance levels seriously inclines him to make errors of this kind. We shall show that that is precisely what Krehbiel has done in the case for which his evidence against committee bias appears strongest.

Testing for Preference Outliers: A Detailed Look at Senate Agriculture

Having taken issue in the last section with the data and specific tests that Krehbiel employs to investigate committee bias, we shall show how different evidence and more appropriate tests suggest quite different substantive conclusions. While the type of data we use will not be appropriate to tests for all committees, our approach does reveal the characteristics of evidence and analysis that are necessary for one to determine whether committees are preference outliers in the ways that assignment theory predicts. The first and most important point is that one must be able to infer policy-relevant biases from the data without making theoretically unreasonable assumptions about measurement error. Although this is seldom easy to do, we believe that attention to this principle will prove central to future attempts to evaluate alternative theoretical accounts. Second, the data must be, at the very least, jurisdiction-specific and, where jurisdictions are particularly diverse, issue- or program-specific. Finally, one should pitch the investigation at the proper level of institutional analysis or, when in doubt, at each of several plausible levels in order to evaluate the robustness of the tests.

To illustrate our approach, we focus on the Senate Agriculture Committee for the Ninety-ninth Congress. In choosing this focus, we do not mean to suggest that Agriculture is a difficult test case for some general outlier prediction that might thereby serve to rehabilitate the stylized view of biased committees. As we have already suggested, we strongly believe that there are sound empirical and theoretical reasons for rejecting such an account a priori. In fact, Agriculture is one committee that, given the widely acknowledged constituency orientation of its members and the relative homogeneity of its jurisdiction, both empirical and theoretical scholars are apt to predict a finding of significant policy bias.

Rather, the choice of Senate Agriculture is particularly appropriate for our purposes in that it is a case specifically biased against our critique of Krehbiel's analysis and conclusions, most of which deal with House committees. First, by focusing on the Senate, we dramatically reduce what serves as the sample sizes for the hypothesis tests, given that the Senate membership is less than one-fourth that of the House and that the average committee membership in the Senate is less than one-half that in the House. (House Agriculture numbered 43 in the Ninty-ninth Congress, Senate Agriculture 17.) Hence, the ability to achieve statistical significance using Krehbiel's tests (even though we are dealing with the full populations of both chamber and committees) is necessarily diminished. Second, given that the Senate committee system constitutes a less differentiated division of the total policy domain (16 standing committees versus 22 for the House) and given that Senators' constituencies are clearly more heterogeneous, we should expect that an accurate matching of member interests to assignment portfolios will be inherently more difficult. Finally (and most importantly), Krehbiel specifically concluded that the Senate Agriculture Committee was "somewhat (but not significantly) more *anti*-agriculture than the Senate" (1990, 158; emphasis in original)—an anomalous result the likes of which he reported for no other committee. In short, should our evidence suggest that Senate Agriculture is significantly *pro*-agriculture, the validity of Krehbiel's tests for other committees, especially those in the House, will be called into serious doubt.

In order to investigate the possible policy biases of Senate Agriculture, we rely on data that reflect the economic interests of committee members' and nonmembers' constituents in committee programs (see also Arnold 1979, chap. 6; Cowart 1981). Employing such data requires that we make two explicit assumptions. First, we assume that differences in members' preferences are, in large part, based on differences in their geographic constituencies. While we do not believe that this is a reasonable assumption for all issues in all committees, we do believe that it is relatively noncontroversial in the area of agricultural policy.

The second assumption has to do with the important distinction between preferences and interests. In general, indications of constituent interest in a program or jurisdiction mainly suggest that the member will be interested and therefore active (Hall 1987); they do not necessarily tell us what policy position he or she will prefer. Whether one can make inferences about preferences from constituency data depends upon the nature of one's indicator and the extent to which there is significant division of policy opinion among interested constituents or subconstituencies. For instance, one cannot assume that public lands acreage in a member's district necessarily evokes constituents' conservationist attitudes; it might reflect private development pressures on the member as well. More generally, it is not always obvious how members' complex perceptions of their constituencies (e.g., geographic, electoral, partisan) affect their preferences on specific issues (Fenno 1978). In order to make inferences about member preferences, therefore, one may require more refined measurement reflecting the balance of different interests or the inclination of the member to respond to one rather than another (Grofman, Griffin, and Glazer 1990).

Fortunately for our illustrative purposes, such complications are relatively modest in the agriculture case. We employ here two general indicators to capture senators' preferences for federal aid to agriculture: (1) total U.S. Department of Agriculture program payments to the senator's state measured as a percentage of gross state product and (2) state agricultural debt-asset ratios. The first reflects the stake of the state economy in the maintenance and/or expansion of federal crop subsidies and other agricultural programs; we assume that ceteris paribus, senators from states affected by such programs will prefer proposals that provide more such benefits to those that provide less.[5] The second captures the severity of the agricultural debt crisis—the most pressing issue on the agricultural agenda during the Ninety-ninth Congress. In using this indicator, we assume that the greater the severity of the debt crisis in a senator's state, the more willing he or she will be (ceteris paribus) to support federal aid to distressed farmers in the form, say, of more generous income maintenance, Farm Credit System relief, or institutional reforms to postpone or prevent farm foreclosures. In addition, we employ similar indicators of constituency interest in several of the specific programs that fall under the committee's jurisdiction, so that we can test for hypothesized subcommittee-specific and issue-specific biases that might be masked by the more general indicators. The program-specific indicators are federal feed grains program payments, wheat program payments, dairy production (wholesale sales), conservation expenditures, and food stamp program expenditures.[6]

In sum, our data should permit us to conduct a set of hypothesis tests tailored to the specific hypotheses and levels of analysis that committee assignment theory suggests, while avoiding the inherent biases of roll call–based measures. In Table 2, we use the constituency-based measures of preferences to replicate three of Krehbiel's tests: the percentage of members in the gap between committee median and chamber median (including committee members), the t-statistic for the difference between committee mean and chamber mean (excluding committee members),[7] and whether or not the committee is *homogeneous* in the sense that the committee's Democratic and Republican median members are on the same side of the chamber median. As the table shows, the Senate Agriculture Committee is clearly *not* antiagriculture. Indeed, it

Table 2. Constituency-based Preferences and Committee Bias: Evidence from the Senate Agriculture Committee

Preference Indicator	Percentage in Gap between Medians	*t*-statistic for Difference in Means	Party Medians
General indicator			
Agricultural program payments	28	2.69	homogeneous
State debt-asset ratio	10	2.66	homogeneous
Program-specific indicators			
Feed grains payments	14	2.59	homogeneous
Wheat payments	6	1.87	homogeneous
Dairy production	6	.69	homogeneous
Conservation payments	18	2.02	homogeneous
Food stamp expenditures	14[a]	.49	heterogeneous

Note: Indicators of committee member and nonmember constituency interests were calculated from state data reported by the Department of Agriculture and the Census Bureau for 1984. N of Senate Agriculture Committee for the Ninety-ninth Congress = 17.

[a] The panel rates *lower* than the chamber.

would require a strong repudiation of the constituency-representative connection to conclude that it was anything but *pro*-agriculture. Despite the small effective sample sizes, Senate Agriculture members are significantly more predisposed to support agriculture spending and crisis relief measures: the difference-in-means test reveals differences that are statistically significant at the .01 level for both of the general indicators, and both reveal the committee to be homogeneous according to Krehbiel's relatively demanding party medians test. While it is not easy to know what standard to use, there also appears to be a substantial number of members in the gap between committee and chamber medians. In interpreting the numbers in column 1, however, it is important to keep in mind that it is by definition impossible for more than 40% of the Senate's members to fall in this gap (roughly, half the total chamber membership minus half the committee membership).[8] Given that upper limit, the fact that 28% fall in the gap between medians for the total program payments indicator is striking indeed.

The program-specific indicators provide similar evidence of committee bias, yet they also reveal the importance of developing and testing program-specific hypotheses even for committees that, like Agriculture, exhibit relatively little jurisdictional diversity. The differences in committee-chamber means are statistically significant at .05 for three of the four agricultural program indicators, and they reveal the committee to be homogeneous without exception. As we note above, however, the food stamp program stands out as the principal exception to a jurisdiction that is generally concerned with programs for rural areas; committee assignment theory therefore does not predict that food stamps will enjoy proprogram bias. The results of Table 2 bear this out. Committee members' interests along that policy-specific dimension appear heterogenous; and, using an appropriately higher alpha level (e.g., .25, *t*-statistic = .68) to assess a negligible-difference hypothesis, we find that the hypothesis is supported. Judging from a comparison of committee and chamber medians, in fact, there appears to be some constituency-based committee prejudice *against* the food stamp program. To the extent that this is true, at least, any summary measure of Agriculture Committee program preferences that combines, say, food stamps and feed grains is apt to hide biases that may have important consequences for committee deliberations about one or both programs.

Table 3 explores the robustness of our results at other levels of institutional analysis. Despite the smaller effective sample sizes (ranging from 5 to 13), such tests provide additional evidence of policy bias on Senate agriculture panels. The evidence is particularly striking for the Senate Agriculture Subcommittee on Production, Marketing, and Price Stabilization—the subcommittee with jurisdiction over all of the commodity programs and most of the committee's discretionary budget authority. Each of the relevant in-

Table 3. Constituency-based Preferences and Subcommittee Bias: Evidence from Senate Agriculture-related Panels

Panel and Preference Indicator	Percentage in Gap between Medians	*t*-statistic for Difference in Means	Party Medians
Agriculture			
Production, Marketing, and Prices Subcommittee (N = 12)			
Agricultural program payments	34	3.65	homogeneous
Feed grains payments	34	3.14	homogeneous
Wheat payments	22	2.48	homogeneous
Dairy production	20	1.52	homogeneous
Credit and Rural Development Subcommittee (N = 7)			
State debt–asset ratio	6	1.63	homogeneous
Research, Conservation, and Forestry Subcommittee (N = 10)			
Conservation payments	16	1.08	homogeneous
Nutrition Subcommittee (N = 7)			
Food stamp expenditures	22[a]	1.93[a]	homogeneous[a]
Appropriations			
Subcommittee on Agriculture and Related Agencies (N = 13)			
Agricultural program payments	28	4.13	homogeneous
State debt–asset ratio	18	2.74	homogeneous
Small Business			
Subcommittee on the Family Farm (N = 5)			
Agricultural program payments	2	.63	heterogeneous
State debt–asset ratio	28	2.30	homogeneous
Membership on any agriculture-related panel (N = 32)			
Agricultural program payments	28	4.06	homogeneous
State debt–asset ratio	14	4.82	homogeneous

Note: Indicators of senators' constituency-based preferences were calculated from state data reported by the Department of Agriculture and the Census Bureau for 1984.

[a] The panel rates *lower* than the chamber.

dicators reveals that the subcommittee is homogeneous; that a substantial number of members fall between the subcommittee and chamber medians; and with the possible exception of dairy (statistically significant at .10), that there is a substantial difference in panel–chamber means. The evidence for Agriculture subcommittees likewise reinforces our argument that any analysis that fails to distinguish between programs within particular committee jurisdictions stands to mislead us: unlike any other program in Agriculture's jurisdiction, the Nutrition Subcommittee is, in terms of consistency-based predilections, homogeneous and biased against its principal program.

In addition to the evidence for Senate Agriculture subcommittees, we also note that two other standing committees have subcommittees whose jurisdictions deal almost exclusively with agriculture issues. Hence, the failure of a member to seek or receive assignment to Senate Agriculture does not necessarily mean that a self-selection-driven, proagriculture bias does not materialize elsewhere. The two general indicators reveal the Agriculture Appropriations Subcommittee to be a homogeneous high-demander with respect to agriculture programs. In addition, the indicator that reflects constituency interest in the single issue of greatest importance to family farmers during the mid-1980s—the severity of the agricultural debt crisis—suggests that the Small Business Subcommittee on the Family Farm was also homogeneous and biased. Finally, when one tests for the bias evident in the subset of members who receive assignment to one or more of the several agriculture-related panels, the results overwhelmingly support the self-selection account.

In sum, we provide consistent evidence at several levels that the Senate Agriculture Committee and the several subcommittees with agriculture-related jurisdictions are homogeneous and biased in ways that are likely to affect specific committee decisions, evidence that stands in stark contrast to Krehbiel's roll call–based analysis of the same committee during the same period.[9] The strength of our evidence, of course, is itself dependent on contestable assumptions. One possible objection is that similarities in constituents' stake in particular programs may mask differences in what they think should be done (and hence in representatives' perceptions of constituent opinion). Both within and across, say, corn-producing districts, there can be substantial differences within the agricultural community on questions ranging from payment limitations to conservation compliance. We do not wish to claim otherwise, and we thereby recognize that such indicators necessarily contain measurement error. Even so, they should still capture well the member's predisposition to value federal actions or expenditures providing net benefits to agricultural producers (and to discount the net costs to taxpayers or consumers); and these predispositions can affect any number of minor and major, visible and invisible choices made during committee deliberations. To the extent the reader finds this assumption implausible, however, our results will be less convincing, and the prospects for using similar data in the study of committee bias less promising.

A second possible objection to our data owes to the fact that even on agriculture issues, preferences can be based partly on nonconstituency considerations, a general point that we also concede. Following Arnold, we adopt the view that the member's "true preference" is based on both general-benefit and local-benefit evaluations (1979, 39). By relying on data that tap district economic stake in a program, we measure only the local-benefit component of the member's preference.[10] Three arguments suggest that this limitation does not seriously undermine our specific conclusions, however. First, our analysis need not assume here that constituency considerations push out all others, only that they constitute an important variable in the preference calculation; that is, we simply assume that members' preferences are a function of some set of variables, one of which is district economic interests, and that the coefficient on that variable is positive and significant. This we take to be a more-than-reasonable assumption. Indeed, a wide range of other accounts reveal that the preeminence of constituency considerations seems especially applicable to the agriculture area (e.g., Jones 1961; Kingdon 1981, 123, 232; Smith and Deering 1984, 105–6, 116). Even, say, the atypically pure ideologue Jesse Helms strongly supports heavy-handed federal interventionism in the tobacco economy, where the financial interests of his constituents are at stake.[11] What we have shown, then, is that the values on the constituency variable for committee members are significantly different from the values of committee nonmembers, applying the appropriately demanding thresholds of statistical significance. Second, the homogeneity tests reported in Tables 1 and 2 demonstrate that members of both parties (and hence quite different ideological dispositions, regions, and group affiliations) appear biased on constituency grounds—that is, even were the chamber to be composed solely of Republicans, there would exist strong constituency pressures for committee members, relative to nonmembers, to pursue policies generous to agriculture interests. Third, we repeated the chamber–committee difference-in-means tests by party and found that for every indicator, both general and program-specific (except food stamps), the respective party means on the committee were greater than the party means off the committee and that in almost every case, both party's committee–chamber differences proved statistically significant at the .05 level. In short, the constituency component of members' policy preferences is significantly biased with respect to committee membership even if the institution does constrain committees—through party ratio requirements—to have ideological diversity that tends to approximate the chamber.

The defense of our assumptions in the Agriculture case is not intended to recommend the indiscriminate use of similar data to test for committee bias, however. If anything, it illustrates how difficult it is to infer members' preferences from publicly accessible data; Senate Agriculture was convenient for our check of Krehbiel's analysis precisely because of the highly plausible connection between member preferences and available constituency data. At the same time, such data partly capture an outcome-relevant dimention of committee–chamber differences that Krehbiel discusses but voting data do not tap—preference intensity. The extent of the district's stake in a particular issue or jurisdiction is likely to affect not only members' policy positions but also the vigor with which they pursue their preferred outcomes in the legislative process. In fact, such differences in intensity are likely to affect forms of participation far more important (and far less observable) than the casting of roll call votes (Hall 1987, 1990). Specifically, such differences will affect the relative willingness of members to expend time, staff, and political capital in setting the legislative agenda, developing amendments, building coalitions, and a wide range of other activities at the committee, floor, and postfloor stage. Differences in preferences, in short, are not all that we need to know.[12]

Conclusion

The idea that committees tend to be unrepresentative of their parent chambers is central to much of what legislative scholars write and teach about Congress, to the strategies that interest group lobbyists and agency officials develop to pursue their legislative objectives, and to proposals long advocated by democratic reformers. The recent work of Krehbiel (1990) thus stands as an important challenge to conventional ways of thinking about Congress and, if correct, should significantly reorient the agenda for both empirical and theoretical research.

We have reinforced Krehbiel's challenge, arguing on theoretical grounds that the stylized account of committee bias mischaracterizes the nature of legislative organization. At the same time, we contend that Krehbiel's revisionist view—that preference outliers are rare—does the same. We elaborate a more differentiated set of conditions under which we should predict committees to be unrepresentative of their parent chambers; challenge the data and methods used to assess outlier claims; and, providing a detailed examination of a single jurisdiction for which we predict specific and variable policy-relevant biases, illustrate the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization.

Neither our general critique of Krehbiel's approach nor our own empirical analysis, then, is intended to suggest that all committees will be composed of preference outliers—much less for all issues within their jurisdictions. A number of long-standing institutional constraints limit committee self-selection; and these constraints are consistent with the important and theoretically innovative perspective being developed by Gilligan and Krehbiel (1989, 1990) and Krehbiel (1989), which suggests that a majority-rule institution achieves important informational gains by limiting committee bias. Indeed, once such constraints are in place, the majority cannot only allow the interest-driven initiation of assignment requests by individual members, it can also allow advocacy and accommodation to characterize the behavior of zone representatives, members of the committees on committees, and party leaders—and committee bias is *still* going to be kept within some bounds. However, the theoretically significant question then becomes, To what extent, in what particular ways, and under what conditions do significant work group biases appear, given the constraints?

Our analysis suggests that several factors, often acting in combination, will affect the extent to which preference outliers will dominate specific panels on specific issues. In particular, three deserve greater attention in future research: (1) the extent to which the panel's policy jurisdiction is narrow and homogeneous, (2) the identification of specific issues within the panel's jurisdiction that evoke the concerns of a mobilized or otherwise visible constituency, and (3) the identification of issues within the panel's jurisdiction that are perceived as having concentrated benefits and dispersed costs. The first two of these follow from the extensions of committee assignment theory we have developed. The third follows from what we believe to be a necessary modification of Niskanen's stylized view of legislatures, that "advocacy is concentrated and opposition is diluted" (1971, 139). This characterization, we believe, should not be an unqualified assumption but a variable, the value of which will affect whether a committee will be biased and, if so, for which specific issues. To the extent that Niskanen's premise holds (as it does for the most part in the area of agriculture policy making), self-selection should produce a policy bias. Where both advocacy and opposition are concentrated (as in the case of, say, energy policy), self-selection is more prone to produce a committee that is roughly representative of the parent chamber.

Thus, the collection and analysis of better data relevant to other committees should not lead to findings of substantial bias for all committees and all issues. But our critique does suggest that where committee bias exists, Krehbiel's approach will either fail to find it or understate it. In so doing, we fear that it pushes the study of legislative organization away from one caricature, that biased committees are ubiquitous, toward another, that biased committees are rare. At the same time, however, Krehbiel's challenge provides a badly needed reminder (one needed in areas well beyond the study of Congress) that stylized accounts can distort as much as inform, that strong tendencies to reify these accounts are evident in the literature, and that such reification can misguide theoretical inquiry.

In sum, while we recognize that stylized facts are necessary simplifications in the formal modeling of institutions, especially in the early stages of theory development, the next important step is to build on more sophisticated and systematic empirical work to inform the modeling enterprise. We have focused attention on party ratio requirements and other institutional constraints that limit self-selection, suggested conditions under which bias is likely to appear, and addressed significant problems of data and method in the hope of aiding future efforts in that direction.

Notes

We have benefited from the comments of John Chamberlin, Doug Dion, John E. Jackson, David C. King, John Kingdon, Keith Krehbiel, Larry Mohr, and Steve Smith and the research assistance of Gary Levenson and Gary McKissick.

1. It is important to note that in related work with Gilligan (Gilligan and Krehbiel 1989, 1990; see also Krehbiel 1989, chap. 3) Krehbiel develops an informational theory of legislative organization predicting that legislatures will generally prefer unbiased committees because of their relative willingness to share policy expertise but also that a legislature will permit outlying committees under one exceptional condition. The revisionist empirical work with which we take issue here (Krehbiel 1990) is more directly concerned with testing the *conventional* stylized account of committee bias, not Gilligan and Krehbiel's alternative theoretical perspective.

2. It is also the case that the effective work groups that shape policy are often biased through a process of selective participation, which may or may not correspond to formal jurisdictional boundaries (Hall 1990). We confine our attention to the formal panels here, however, since they are the focus of existing institutional analysis though the examination of committee or subcommittee memberships is not dispositive with respect to the larger issue of work group bias.

3. In a larger study, Krehbiel focuses his tests at the subcommittee level for one of his broad scope committees, House Appropriations, but does not generalize this research strategy to any other committees (1989, chap. 4).

4. Although only votes involving issues *clearly* beyond the boundaries of the Agriculture Committee were eliminated, these accounted for 8 of the 24 votes that NFU selected: 3 of the 12 in 1985 and 5 of 12 in 1986. The votes eliminated for 1985 were a resolution dealing with MX missile appropriations, a joint resolution to raise the debt limit and approve the Gramm-Rudman balanced budget amendment, and adoption of the conference report to raise the debt limit and approve Gramm-Rudman. The votes eliminated for 1986 were passage of a joint resolution to propose a balanced budget amendment to the U.S. Constitution; an amendment to approve aid to the Nicaraguan Contras; an amendment to prevent the deregulation of old natural gas; an amendment supporting the deductibility of state and local taxes for federal tax purposes; and an amendment to reform campaign finance regulations for congressional campaigns. See "1985 Voting Record" 1986a and "1986 Voting Record" 1986b.

5. We use data on federal program payments on the assumption that they best capture the state's stake in the program and hence indicate the member's likely "demand" for program benefits. An alternative approach is to use general and crop-specific measures of agricultural production. We replicated our tests using such data; and in every instance those results provided evidence of a pro-agriculture bias at least as strong as those we report in Tables 2 and 3. By using the program payments data, then, we employ an indicator that we believe makes theoretical sense; but in any case, the choice of program over production data does not affect any of our general or specific substantive conclusions. On the contrary, the fact that the two different kinds of data produce similar results reinforces our principal conclusions.

6. Data on agricultural debt-asset ratios, total federal program payments, and (with the exception of food stamps) the program-specific indicators are based on data from *Economic Indicators of the Farm Sector, State Financial Summary* for 1984, the year immediately preceding the period during which

committee assignments for the Ninety-ninth Congress were made. Data on state food stamp expenditures were taken from "Federal Expenditures by State" in the Bureau of the Census: *Governments Annuals and Biennials.* We use dairy production data rather than dairy program payments data to assess the extent of a prodairy bias because, due to the indirect nature of dairy subsidies, data on payments going to state dairy producers are not available. With the exception of agricultural debt-asset ratios, each indicator was defined as a percentage of gross state product, the data for which were taken from "Gross State Product by Industry, 1963–1986," in the Bureau of Economic Analysis' *Current Periodicals* for 1988.

7. Given that we intend to draw a conclusion of *difference,* it is incumbent on us to adopt a demanding statistical standard, i.e., a low alpha level. We focus at the .05 level, one-tailed, which requires a *t*-statistic of approximately 1.66, though we also note when key results pass either the .01 or .10 thresholds (*t*-statistics of 2.36 and 1.29, respectively).

8. For the House, the maximum percentage in the gap averages closer to 45%.

9. In addition to the tests reported here, we also conducted tests for committee chamber differences for several more minor programs. Even using these indicators, additional evidence of a self-selection bias appeared. For instance, the difference in means between chamber and committee was statistically significant at .10 for both rice and cotton programs, while the difference in means between the chamber and the subset of members who held assignment to any agricultural panel was statistically significant at .05 for these same two programs. (However, the difference was correct in sign but statistically insignificant for Wool Act payments, regardless of the subset analyzed.) In addition, comparable tests using agricultural production rather than program payment data produced statistically significant differences between chamber and agricultural panel members for feed grains, rice, wheat, cotton, peanuts, and soybeans, as well as for all agricultural crops combined. The only major agricultural products subsidized by the federal government (and for which we had production data) that failed to exhibit a statistically significant panel–Senate difference were wool and tobacco.

10. In addition, our constituency indicator fails to capture constituents' preferences for public goods (Jackson and King 1989). With the exception of nutrition policy (which evokes some constituents' preferences for public attention to basic needs and other constituents' preferences regarding governmental waste, fraud, and abuse) and certain environmental issues with Agriculture's jurisdiction (e.g., pesticide use, groundwater pollution), public goods preferences are seldom prominent in the dialogue between the member and the subconstituency that takes an interest in his or her actions in the agriculture area.

11. Note that the Helms example provides us not simply with anecdotal evidence but a limiting case: if the prototypical ideologue suspends his ideological predispositions when constituents' agricultural interests are salient, it is relatively safe to assume that other members do as well.

12. We thus reaffirm Krehbiel's emphasis on the direction of preference/intensity of preference distinction; but we strongly disagree with his assertion that "if preference outliers . . . do not exist . . . then the existence of intense or high salience outliers would be inconsequential for the ultimate distribution of policy benefits" (1990, 5). In fact, this assertion holds only in the unusual case in which the distribution of intensities mirrors the distribution of preferences; that is, in the determination of policy outcomes the teeter-totter principle holds: the outcome depends on the ratio of weight times distance and weight times distance. Krehbiel asserts that only distance matters. Physical science holds otherwise, and political science suggests that, in the United States at least, intense minorities' preferences usually rule, not majorities' (Dahl 1956, chap. 5).

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BARGAINING WITH THE PRESIDENT: A SIMPLE GAME AND NEW EVIDENCE

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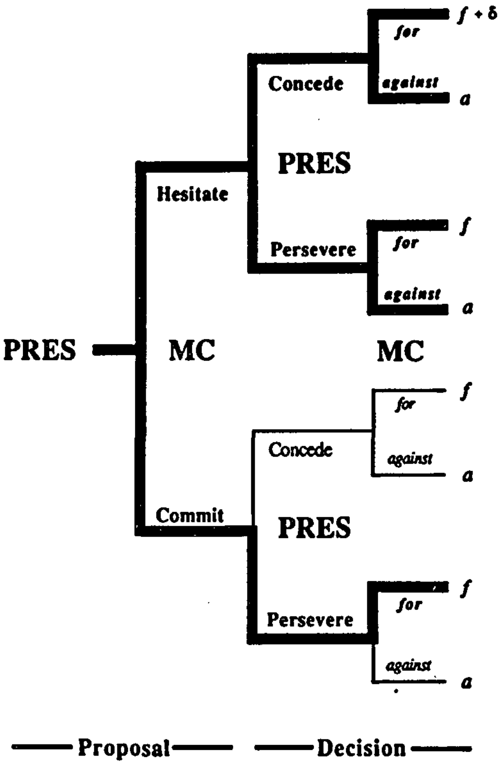
I employ a simple game to suggest the effects of information and expectations on bargaining strategies and responses. I make use of members' predispositions in order to identify actors likely to be bargaining. Using administration headcounts, I show that while very few members misrepresent their preferences during the coalition-building process, those who do represent a large proportion of the administration's core supporters, make their misrepresentations unsystematic to avoid a costly reputation, and convert more readily than those who are not. Strategic considerations drive conversion among bluffing members, while identification with the administration determines conversion among other members. Compromise generates few conversions. A conservative estimate of bluffing suggests that the conversion of bluffers decided more than half the administration's critical votes. I speculate about a model to account for the observed bluffing.

Since Richard Neustadt's *Presidential Power* (1960) presidential scholars have accepted the observation that the president's power is the power to bargain successfully (p. 36). Yet since then, two problems have stymied efforts to expand Neustadt's observation into a research agenda. First, while many believe that bargaining matters, no one has developed a formal theory suggesting how it matters. Second, despite its obvious appeal,[1] all the discussion of bargaining, by analysts and politicians, flies in the face of the empirical research. For example, Edwards (1980, 1989) assesses presidential influence utilizing a variety of standard measures (e.g., presidential "batting averages" and "support scores"). He concludes that "presidential legislative skills do not seem to affect support for presidential policies, despite what conventional wisdom leads us to expect. Sources of information about presidential-congressional relations, particularly the press, seem to have focused upon the more unique examples of these relations, implying that what they are presenting was typical. When we rigorously and systematically evaluate the evidence, however, we reach different conclusions" (1980, 202). Instead, Edwards concludes that presidents rely on advantages more directly linked to electoral resources, such as party ties, coattails, popular prestige, and so on. While at variance with the practitioner's view, the appeal of these empirical results is unmistakable. If reelection drives members of Congress, the president's influence with these members depends upon their shared electoral "fate." This linkage would define the president's bargaining ability (see also Kingdon 1981, 199). Of course, the typical empirical approach to presidential influence actually estimates a model of presidential "success" in floor voting. With a specification more closely mirroring the coalition-building process, it is

Figure 1. A Simplistic Model of Bargaining



possible to discern what clearly are skill effects. Regardless of the accuracy of these past empirical analyses, however, no research has attempted to develop Neustadt's insights by modeling presidential-congressional bargaining itself. Without such a model, any speculation about the importance of presidential skill and influence remains just that.

My research agenda on presidential influence explicitly proceeds from a bargaining perspective. First I shall present a simple game representation of what Neustadt describes as the "mutual dependence" between presidents and members of Congress and discuss five reasonable empirical expectations about congressional bargaining with the president. I shall then identify member predispositions and describe new data on the actual prevoting positions of members. Using these data, I shall test the empirical expectations. The test results will help identify an opportunities model that is consistent with what actually happens and confirms the importance of the Edwards results yet demonstrates that bargaining matters.

Utilizing a Simple Game of Mutual Dependence

The foundation of Neustadt's familiar analysis is the "mutual dependence" between president and Congress, which affects their separate prospects for power (1960, 356). On the one hand, the president's ability to obtain the support of some congressional majority limits presidential fortunes; on the other, the president's influence tempers the power of every congressional majority. In developing this theme, even a simple game model of bargaining can be instructive. Figure 1 illustrates a game involving only two players: the president and a single member of Congress. This game proceeds in two stages:[2] a proposal stage initiated by the president and a decision stage dominated by the member. In this simplistic game setting, then, the president proposes and the Congress disposes. Since each cannot complete the game without the participation of the other, the two are mutually dependent.

In the decision stage and for reasons of simplicity, the model assumes that each player has only two options. The president either sticks to the current administration proposal or grants some concession. The member must decide either to vote *for* or *against* the current proposal. These options result in one of two outcomes, *f* (supporting the president) or *a* (opposing the president). In the proposal stage, the president initiates a proposal to begin the game, and only the member has two options: one signals a commitment to the president's proposal, while the other signals some hesitancy about supporting the proposal.

While the outcome rests on mutual dependence, one other thing affects how players act: how much information is available. The simplistic model presented so far assumes that each knows the other's preferences and available responses (i.e., each player possesses complete information). In this simplification, what the president should do, once presented with the member's signal, will depend on the president's assessment of the member's preferences about voting. If the member prefers the proposal (i.e., prefers *f* to *a*) and hence will vote for the proposal anyway, there is no need to concede a favor even when the member is hesitant. If the member prefers not to have the proposal (prefers *a* to *f*) and hence will vote against the proposal even if the president concedes a favor, there is *still* no reason to concede. Of course, given complete information, the president can accurately assess how the member will vote. And the member knows that the president knows how the member will vote, and the president knows the member knows that the president knows, and so on.

In the end, this simplistic game, which models mutual dependence and focuses on information, generates two suggestive results. First, complete information precludes some choices. For example, a member has no incentive to vote against after initially committing even though such behavior is logically possible. Likewise, when the member initially commits, there is no need for the president to consider conceding, even though that too is a logical possibility. Thus, given complete information, a member's initial choice of committing results in *f* as a foregone conclusion. Hence, the game tree illustrates a single path (one of the heavy lines) down the *commit* side of Figure 1. Second, this simplistic game model generates a very straightforward equilibrium: the president will concede only when that concession will result in changing the hesitant member's vote from *against* to *for*. Given that the member knows this is what the president has deduced, the member will hesitate only when a concession is necessary to change that vote. Thus, this simple model underscores the important linkage between what the member signals in the proposal stage and all of the choices either possible or made thereafter. It suggests further that the strength of that linkage between signaling and choice might be central to bargaining.

There are two possible research strategies for elaborating the simplistic game in order to assess bargaining. First, a researcher might develop new theories incrementally by adding more complex forms to the simple game model (see Sullivan 1988a). Second, a researcher might examine how presidents and members actually bargain in hopes of motivating appropriate extensions of the simple game. The first approach requires surmounting several theoretical problems with which theorists are grappling. The second approach focuses on theoretically important questions, but it also requires further empirical development. For example, it requires developing a technique for estimating the predispositions of members. It also requires the data to track a member's behavior prior to voting. The research reported here follows the second strategy. By utilizing a new technique it estimates predispositions. By utilizing new data, it tracks how different members bargain with the administration. In doing so, the research highlights two closely related elements familiar to the Neustadt perspective: expectations and reputation.

Expectations and Reputation

No bargaining takes place in the simple game, because information is complete; that is, each player knows the preferences of the other, and each knows that the other knows that. Of course, no matter how astute and experienced they are, real politicians do not know for certain the preferences of their fellows. In game-theoretic parlance, they possess incomplete information. Through their choices, however, players can still signal something of their intentions to the others even though that signal is now less revealing. Via those signals, each participant still possesses some control over what others may expect and hence over the final outcome (see also Neustadt 1960, 84). Yet, with the addition of uncertainty, the linkage between signal and choices is far more complex. For example, without preference information, a member's hesitation might convince the president that the member prefers to vote *for* the proposal only if given some concession (i.e., the member prefers $f + \delta$ to a and a to f) when the member actually prefers the proposal. The president's bargaining problem is how to interpret hesitation and discern whether it results from a genuine reluctance to support; that is to say, the administration's job is to decide when hesitation necessitates haggling and when it is purely bluff. Fortunately for the administration, the lack of information also hampers the member. The member is never quite sure what impact initial hesitation might have on the president's calculus and how much information the president has about the member's true preferences. For that matter, incomplete information means that the member is never quite sure whether δ might not be too much to ask for (i.e., whether the president might not prefer a to $f + \delta$ even though surely preferring f to a). So there is no straightforward prediction about what course is best for the president (to concede or to persevere) even when the member is hesitant. Hence, there is no straightforward prediction about what course is best for the member (to hesitate or to commit).

Adjusting strategy selections in this new, uncertain situation involves some complicated processes (see Ellsberg 1956). For example, if the member expects that the president (deciding in ignorance) will always concede when the member hesitates, the member might always hesitate. The president, on the other hand, may suspect (or may learn) that in some (maybe in all) situations a concession is not necessary to obtain a *for* vote: the member bluffs hesitancy. As a result, the president might adopt different estimates of how important granting a costly concession really is. The president could decide never to concede, even though refusing to grant favors undoubtedly risks missing out on some of the potentially available support. Conversely, when the two interact over many issues the president might devise a strategy designed to punish a bluffing member (by withdrawing credit, for example) so that over time the total gains from bluffing would be zero. In turn, the member contemplating bargaining might adjust prior estimations of the president and seek some middle ground where the member rarely employs bluffing and hesitancy is typically genuine. Then the president might adjust in response, to make punishment rare as long as hesitancy *appears* genuine.

Thus, for both the member and the president, one important aspect of bargaining is a professional reputation defined as an expectation about the linkage between one's signals, preferences, and ultimate actions. When information is incomplete, reputation and the expectations they engender act as a force analogous to (but of course not quite identical to) complete information. Maintaining a reputation over time eliminates specific options by making unreasonable some strategies that are otherwise perfectly allowable. Also, as repetition confirms reputation, firmed expectations portend the appearance of an equilibrium similar to that under complete information, balancing signals and actions. The actual characterization of this equilibrium, however, is very complex (Kreps and Wilson 1982); and it often suggests some unusual behavior. For example, circumstances might force the president or a member to do something unreasonable in the short run in order to maintain a specific reputation. In particular, the need to preserve a reputation might force a member to carry through on a threat when the administration does not respond favorably.

Thus, the loss of complete information means that the ability to send signals, which was so critical to stabilizing the outcome in the original simple game, now undermines stability by allowing a player to manipulate the expectations of other players. At the same time, this signaling lays the ground work for a stable relationship, though even this is fraught with temptations. Such complexity is an important possibility both in developing bargaining theory with incomplete information and in the actual practice of leadership. Thus, assessing how often members of Congress misrepresent their support with their signals in the real bargaining process is an important empirical (as well as theoretical) consideration.

Empirical Expectations

Although the previous discussion did not develop a formal theory, it does suggest the following expectations about bargaining:

1. *The Infrequency of Misrepresentation.* Some, but not many, members initially misrepresent their positions as hesitancy.

2. *The Importance of Avoiding a Bad Reputation.* A diagnostic model performs poorly in predicting which members will respond with initial hesitancy. In addition, to the extent that any of their hesitancy is systematic, variables defining the bargaining situation are better descriptors than variables describing the member.

3. *The Absence of Unreasonable Positions.* Since it is an unreasonable strategy, members predisposed to oppose the administration do not commit in the proposal stage and then vote against the administration in the decision stage.

4. *The Relative Regularity of Conversion.* The rate of conversion among hesitant members identified under expectation 1 is considerably higher than other conversion rates.

5. *The Strategic Nature of Conversion.* Among members identified under expectation 1, a diagnostic model of conversion identifies strategic variables as significant while the same conversion model for others identifies only descriptors of the member as significant.

The first three expectations stem from the suggestion that members develop a balance between the desire to take every advantage and the necessity for restraint so as to nurture one's reputation. Given that presidential bargaining is a very complex game when it is based on expectations (which are manipulable), some misrepresentation is likely. Interestingly enough, an administration probably invites this manipulation. Presumably, it wants members to know that it rewards those who take "statesmanlike" positions, going against their constituency or their better judgment to make the hard, but nevertheless "right" decisions. Otherwise, the administration cannot expect to tempt potential supporters to cooperate, except on an exclusively quid pro quo basis. This desire to tempt members, of course, leaves the administration vulnerable to being bluffed. Even if members intend to support the administration, they also may consider reporting initial hesitancy, thus creating an opportunity to demonstrate loyalty by later doing something that appears difficult (i.e., converting). In his published diary, for example, G. William Whitehurst (1983, 42) reported an incident in which a Republican colleague waited until the final few seconds before casting his vote supporting a Nixon veto. When approached by Whitehurst to explain his behavior in holding out so long, the member replied, "I just want them to see my name blank up there until the last minute. That way they appreciate me more." Presumably, this member was feigning hesitancy on the administration's headcounts, as well.

On the other hand, several considerations (but certainly three in particular) temper advantage taking so that few ever risk bluffing (expectation 1). First, some situations or combinations of preferences probably make it difficult to pressure effectively. For a bluffing signal to work, it must convey convincing information that contradicts the initial expectations of the administration, suggesting that the member would not be signaling such hesitation unless genuinely reluctant. The normal array of predispositions probably restricts the opportunity for such contradictory yet informative signaling.

Second, successfully pressing the administration for a policy compromise poses a complicated collective action problem. If it is to be successful, a member's demand for concession must usually occur as one among many similar demands. Yet the member is alone and in ignorance of what the others are doing when *reporting* hesistancy to the administration. No member can be sure what response colleagues are likely to make in their private conversations with the administration, especially as the legislative situation underlying any initial agreement begins to change. Members can be sure of the costs they risk incurring, however. Every administration maintains a credit balance with members on which they might draw at some time in the future; often the administration checks this balance before granting requests (Dodd and Sullivan 1979; Holtzman 1970). In political parlance, this credit balance is the value of being a *friend.* Every demand reduces a member's credit with the administration, that is, there is a *friendship cost* associated with each demand regardless of type. Drawing down one's account has the effect of reducing the probability that the administration will grant a favor in the future. With some discounting, then, bluffing has a price even when the member is not caught outright. Also, it might be assumed that there are different kinds of costs associated with different options. For example, in general, producing a public policy concession is strictly more costly for the administration than producing a personal favor. Because of this differential in costs, an administration probably is slower to respond to policy demands unless made by a large group. Hence, every demand carries with it an *organizational cost* in proportion to how many individuals are necessary to make the demand respectable. In the simple game model with only one member, the organizational costs of every kind of demand are equally near zero because the single member's decision on how to vote is critical. But when more members are involved, the administration must be convinced that a net advantage will result from yielding on a publicly taken policy position. Thus, when considering concessions, an administration probably must be convinced that a large faction of members is at risk. And of course, as the number of total members in the game increases, so does the number of coincident demands necessary to trigger a response from the administration. Hence, as the number of members goes up, the organizational costs of coordinating demands for a concession rise dramatically while the organizational costs of demanding a personal favor still remain near zero. The end result may be that no one carries through on their promises to make a concerted demand. It is also difficult to coordinate a joint demand strategy specific enough to enable the administration to recognize what it must do. Eventually, the uncertainties and organizational problems are such that the group probably has to present its demands to the president in person and in detail. Thus, costs plus uncertainty may account for why members often visit the president *in delegations*, in order not only to make clear their demands but also to assure that everyone in the group actually cooperates. Of course, when others are demanding compromise or if the legislative situation is naturally close, members always run the risk that hesitancy might trigger the administration to respond, *but in the wrong direction.* An administration might overreact and withdraw its proposal, for example, thinking that if it had so little initial support it could not possibly win without giving away too much. In this situation, members contemplating a feint in order to demand a compromise might conclude instead that half a loaf is better than none. Since hesitancy always undermines the member's long-term relationship with the administration, adding that loss to the organizing costs may convince each member that a coordinated policy demand is just too costly.

Third, members eschew bluffing in order to avoid developing any one of a number of bad reputations (expectation 2). Presumably, the leadership's high discount on the signal it receives from such members forecloses their future opportunities.[3] Three reputations seem particularly bad. The first two differ only in the member's typical demand, while with the third reputation the balance between gains and costs is poor. The first reputation is that of the member who is always bargaining for a favor regardless of the issue or legislative situation. This reputation is of a member *"We can't count on."* Over time, such a quid pro quo approach develops a negative credit balance with the administration, who, of course, cannot afford always to be conceding favors to anyone. Eventually, say the practitioners, the administration must close the window on these members, shutting them off from administration favors altogether. The second reputation is that of a member who, though constantly demanding compromise, always converts, regardless of the administration's response: a member *Always crying wolf.* One former whip said of a member who regularly opposed the leadership on headcounts in order to pressure the leadership: "Oh, Mr. ______ was always trying to get us to change legislation, to make it more conservative. After a while we realized that he was just pressuring us for some concession. We could always count on his vote, so we stopped being concerned about him. You learn to read a member." For members, the problem associated with crying wolf is not so much that the administration eventually figures them out or gets fed up as that the signaling involved leaves members with no effective way to communicate genuine hesitancy or a need for attention. The only type of unanticipated behavior from these members is committing to the administration, and no member gets the administration's attention by signaling commitment. Members avoid the third reputation, of committing to the administration initially and then voting against it on the critical vote. This reputation is of the member *We can't trust,* and it is the most damaging of all reputations. Just as every administration wants to reward conversion, every administration has an incentive to punish reneging. The member the leadership does not trust risks engendering a dramatic drain of credit. In conjunction with the fact that initially committing to the administration forecloses concession, members gain nothing by bluffing support. One possibility, of course, is that members might bluff support in order to lull the administration into a false sense of security and an ultimate floor defeat. The likelihood, however, is that few members would risk such deception since in a post mortem (and every administration conducts post mortems), the administration surely could discern which members participated. Moreover, since the administration starts off the coalition-building process with far less than a majority, the numbers necessary to lull it into a false sense of security would be very large. In the end then, there are high costs and no benefits associated with pursuing this strategy or developing this reputation (expectation 3). In addition to expectation 3, the argument here also supports another hypothesis. If there were such a lulling strategy, the incidence should concentrate on a few issues; otherwise, given the typically desperate legislative situation facing the administration, no false sense of security could arise.

If there actually are some who feign hesitancy, their behavior in the decision stage could appear complicated. For example, the administration's response is crucial for those seeking favors; it is, after all, a quid pro quo. On demands for policy compromises, the administration's response is less determinate, since when the policy under consideration is important, the member probably will fall into line regardless of the administration's response. Despite this contingent nature, two asymmetries bias conversion behavior. Whether a bluff succeeds or it is called, the member's payoff requires conversion, even despite the potentially harmful reputational effects from folding on a called bluff. As a result, the rate of conversion should be greatest among groups composed largely of bluffing, hesitant members (expectation 4). Two components determine the conversion rate: the first is *natural* conversion driven by predispositions and initiated by either compromise or the introduction of improved information; the second component is *strategic* conversion driven by bargaining considerations, including bluffing strategies or the receipt of personal favors. Using this perspective on conversion components, an analysis of nonfeigning members should highlight basic member or policy characteristics, while on analysis of feigning members should highlight the unfolding strategic situation (expectation 5).

Testing the Expectations

Assessing bargaining requires data tracking member positions throughout the process, estimating member predispositions (in order to identify core supporters, etc.), and identifying specific groups with which to test the expectations.

Available Data

The data used in this research originated in Lyndon Johnson's Office of Congressional Relations (OCR) during the 90th Congress (1967–68). They span 51 different issues (see Table A-1 for a sample).[4] They are a portion of a large data set of over six hundred counts covering five administrations, Eisenhower through Ford (Sullivan n.d.). On most of these issues, the Johnson administration took several counts over a long period of time; the median time between an "initial" count and the critical vote was thirteen days. On its count, the administration recorded members' responses, often including very detailed accounts. These responses include some that evidence hesitancy about the specifics of a proposal. For example on one of the most complicated issues of the Congress, the 1968 tax surcharge proposal, the liaison reported: "I talked today with Mim Daddario. He will not vote for the tax bill conference report; he will vote against the tax bill conference report as he refuses to be 'held up to ransom.' He is strongly in favor of a tax but with the current $6 billion in spending cuts that the conference report contains he is against it."[5] Some members make a specific policy demand. On this same bill, the liaison reported: "Mr. Brademus [*sic*]: Not [supporting] at the present time. . . . What really bothers Brademus [*sic*] is that the surtax package that the administration is pressing for does not contain anything that is calculated to make it politically attractive to the voters of his district. He thinks the tax increase should be packaged with some measure of tax reform. He says that he would not ask for the moon, but just some little "crumb" that he could take to his constituency along with the tax increase. . . . The 'oil fat cats,' etc. and others who have tax havens will not bear the burden of the surtax."[6] Some responses link a member's position to a special favor. For example, on the tax surcharge bill, the liaison reported: "Kelly [NY]: Extremely upset about her redistricting problem. She wants help from the White House to keep Manny Celler from running in her district. She wants a call from the White House on this matter promptly. She won't comment on the tax increase until she hears about her problem."[7]

The OCR then translated these detailed reports and requests into summary categories ranging, in typical Johnson fashion, from *right* to *wrong*. The Johnson administration, like others, felt the detailed responses were useful only in determining which tack to take with a member. For summarizing a member's position, the administration believed the few broad-gauged values on the *right-wrong* scale were best. Moreover, for most considerations of strategy, the administration collapsed even these summaries into two categories: *right* or *in need of persuasion* (i.e. *hesitancy*). This two-valued schema reflects the kind of administration perspective that LBJ aide Douglas Cater (1964) has described: "A President finds himself measuring Congress in terms of the coalitions for him and against him on specific issues" (see also Califano 1975 on the "nonpartisan" presidency). In the final stages of assessing strategic conversions the research introduces the administration's more detailed categorization schema as a control variable. Since the administration was far better at keeping its summary reports than it was at maintaining its detailed reports, I shall focus on assessing the strategic situation as the administration observed it through its count summaries. A later analysis will focus on assessing the administration's more detailed reports.

The data used here indicate that typically about 149 members committed to the administration's proposal on the initial count. This figure jibes with the recollections of the chief OCR officer during the 90th Congress. Harold ("Barefoot") Sanders recalled that the administration "could always start out with 145 to 150 votes in the House . . . , give or take twenty on a particular issue" (Sanders 1969, tape 2, nos. 13–14). It seems clear that Sanders's comments reflect his experiences with what actually happened rather than what he expected based on his "theory" of whence administration support came. Of course, this figure is shy of the 218 votes needed for an absolute majority. The remaining member positions run from *leaning right* through *undecided* to *wrong*. On the typical count, there are usually very few members who respond *undecided*, reflecting the highly charged nature of these presidential priorities. In any case, the administration considers everyone not *right* to be in need of persuasion of some kind.[8]

Empirical Techniques

Before applying data to the simple game-theoretic structures developed here, the analysis utilizes two empirical techniques. The first identifies types of members by specifying a model of member predispositions. The second uses two diagnostic models developed in order to model hesitancy and conversion.

A Model of Predispositions. In order to approximate member predispositions, the analysis employs an "electoral connection" assumption: consistent differences in behavior between members result from differences in their constituencies. To utilize this assumption, a hierarchical cluster analysis identified voting patterns in an earlier Congress (the 89th) (Anderberg 1973; Uebersax 1979). Using a strong parameter of similarity made the clustering more focused than usual, although the resulting clusters were consistent with mainstream research (e.g., Clausen 1973; Sinclair 1982). The analysis identified two clusters as *civil liberties*, and *governing the economy.*[9] A probit analysis then described member voting behavior on these two clusters by calculating parameters on some 26 constituency characteristics (see the Appendix). By applying these coefficients to constituency characteristics updated by forced redistricting prior to the 90th Congress, I produced a prediction about a member's response when a headcount came from an issue group. These predictions represent member predispositions.

The technique defines members as having a strong tendency to hesitate if their probit scores resulted in a probability of supporting the administration of .2 or less ($.2 \geqslant p$). These members are the administration's *core opponents.* Conversely, a probability of .8 or better ($p \geqslant .8$) indicates a strong proclivity to take the *right* position. These members are the administration's *core supporters.* Scores between the two extremes ($.2 < p < .8$) indicate a proclivity to be *cross-pressured,* sometimes supporting the administration, sometimes not (see also Bond and Fleisher 1990, chap. 4; Covington 1988). In the 90th House, the technique identifies 132 members as core opponents, 128 members as core supporters, and 173 members as cross-pressured. On the average initial count, members supported LBJ in direct proportion to their predispositions; that is, very few core opponents responded *right,* while the cross-pressured and core supporters made up the bulk of initial supporters.

The Diagnostic Models of Hesitancy and Conversion. In order to analyze the empirical patterns to hesitancy and then conversion, the analysis introduces two extensions to the basic model of predispositions (see the Appendix). In analyzing whether there is a systematic pattern to hesitancy (expectation 2), the first extension introduces variables controlling for the member's institutional position and for the bargaining situation. In analyzing conversion (expectation 5), the research further enhances the basic model by adding a variable describing the basic direction of administration compromise, as identified by Congressional Quarterly or administration memoranda. It also introduces an ordinal variable that measures the strength of member hesitancy using the administration's more detailed *right–wrong* schema. This variable will control for conversions that result from "natural" hesitation. In order to measure conversion, the analysis compares an initial count position with the closest vote on which the president took a position. The analysis depended on both internal administration documents and the determination of Congressional Quarterly when identifying whether the president took a position. Where the administration and Congressional Quarterly differed, the research relied on administration reports (see also Covington 1987a). The average split on these critical votes was 217 *right* votes to 185 *wrong* votes, an average victory margin of only seventeen.

Table 1. Patterns of Hesitancy by Types of Member Predispositions

Member Type and Predisposition		Predicted As Initially Hesitant[a]		Actual Occurrences of Initial Hesitancy		
	n	p	n	n	Of Group (%)	Difference[b] (%)
$p \leq .2$ (core opponent)	132.7[c]	.91	121.3	120.8	91.0	-.4
$.2 < p < .8$ (cross-pressured)	172.5[c]	.57	98.7	124.6	72.3	26.2
$p \geq .8$ (core supporter)	128.0[c]	.10	13.3	64.2	49.6	382.7

[a] Based on average probit values.

[b] The "difference" is calculated as $(\text{actual} - \text{predicted})_{\text{hesitant}}/\text{predicted}_{\text{hesitant}}$. Differences in percentages of predicted and actual are due to rounding in these two categories.

[c] Averages reflect the changing size of the House membership during the Congress.

Findings

For simplicity of exposition, the tables illustrate the weighted average between the *civil rights* and the *governing the economy* issue clusters. Responses from 25 counts fall into these two clusters during the 90th House, which in turn encompass about ten thousand initial positions.

Do Members Feign Hesitancy?

Table 1 addresses the question of misrepresentation. It reports data on member predispositions and initial position taking. This table distributes the members into the three broad predispositions and reports an expectation about initial position taking. For example, their average predisposition suggests that about 91% of the administration's core opponents should respond with hesitancy. If realized, that percentage would result in about 121 members. Then the table reports the actual levels of initial hesitancy based on observations taken from the headcounts and presented as averages (for core opponents, about 121 members). It reports the proportion the hesitant responses represent of the total group (for core opponents, 91.0%), and calculates the difference between the predicted and actual levels as a percentage of the predicted level of hesitancy (for core opponents, about .4%). In the final column the table summarizes which group or groups are responding in a very unusual way (signaling hesitancy) relative to their normal predispositions.

As is clear, the administration's core supporters are the only group to respond with unusual hesitancy. Based on their generally very weak predisposition to hesitancy (i.e., $1 - \bar{p}_{\text{core}} = .10$), their responses represent a difference of almost 400%! By contrast, the difference between expected and actual hesitancy among the other groups is never much more than 25%. The magnitude of differences between groups is very robust with respect to *p*.[10] Despite the substantial percentage, the number of core supporters adopting hesitation is still a small proportion of the *total* membership (only about 12%). These data, then, strongly support the expectation that few misrepresent, but they also suggest something unexpected, namely, that circumstances or something else limits misrepresentation to the administration's core supporters.

Do Members Avoid a Reputation?

Was it possible for the administration to anticipate the actions of these core supporters? If there is a pattern to feints among the bargaining signals, the admin-

Table 2. A Logit Model of Initial Hesitancy among Core Supporters ($p \geq .8$)

Variables	MLE Coefficient	Standard Error	χ^2
Constant	11.201	2.256	24.65***
Constituency instrument	.003	.010	.09
Institutional context			
LBJ's Gallup approval	-.071	.011	45.37***
Seniority	.002	.003	.51
Democratic leader?	.320	.217	2.19
Total committee value	.059	.103	.33
State of the economy			
Inflation rate	-.010	.006	2.48
Unemployment rate	-.154	.042	13.24***
Strategic situation			
Initial closeness to majority	-.058	.012	22.45***
(Initial closeness)^2	.026	.010	7.33**
Number of cases	2,918		
χ^2_{Model}	257.37		
R^2	.059		
% Correct	59.6		

$^{*}p \leq .05$.
$^{**}p \leq .01$.
$^{***}p \leq .001$.

istration could discount them in anticipation of eventual floor support. Indeed, the earlier analysis suggested that this very prospect makes it unreasonable to expect regular patterns from members. Specifically, the expectation suggests that it would be difficult to predict hesitancy among core supporters.

Table 2 reports a logit model of initial hesitancy among core supporters. Obviously, the model does not perform well, although this specification performed better than any other tested. While not completely comparable with ordinary least squares regression, the logistic regression does report a coefficient of determination (R-squared) that is similar. For this model, R-squared is extremely low at .06. As expected, some variables have significant coefficients; and, as expected, these significant variables characterized the bargaining situation. Variables describing the member generally did not perform well. This result suggests that bargaining (and especially bluffing) is basically unsystematic, as if members were attempting to avoid a bad reputation.[11] In sum, then, there is strong evidence that members balance their responses between taking advantage and restraint. It seems likely that few members misrepresent and that when they do, their bluffing is unsystematic.

Do Members Avoid Unreasonable Positions?

Table 3 reports data on whether members avoid the specific reputation of a member *We can't trust*. It details the numbers and percentages of members who initially commit to the administration and then vote against, along with information about their predispositions. Although the expectation focuses on core opponents, the table reports data on all three groups. Given the average predisposition of core opponents, the model would

Table 3. Initial Commitment and Final Opposition by Type of Member Predispositions

Member Type and Predisposition	n	Predicted as Initially *Right*[a]		Actual Occurrences of *Right/Against*		
		p	n	n	% Group	% Members
$p \leq .2$ (core opponent)	132.7[b]	.09	11.4	1.2	.9	.3
$.2 < p < .8$ (cross-pressured)	187.3[b]	.39	73.0	3.7	1.2	.9
$p \geq .8$ (core supporter)	125.8[b]	.90	113.2	4.6	3.7	1.1

[a] Based on probit values.

[b] Averages reflect the changing membership of the House during the Congress. Total number of cases is 9,880.

suggest that about 9% might actually respond *right* on the initial count. If realized, that percentage would represent 11.4 members on the average count. Table 1 reported that the percentage of opponents taking *right* positions actually averaged 11.9 members, almost exactly that predicted by their predispositions. Table 3 reports the actual numbers of these initial commitments who voted *against*. On average, among core opponents, only 1.2 members actually committed and then voted against the administration. This average represents less than 1% of core opponents, which is about 10% of those opponents initially reporting *right* but also less than 1% of the core opponents, less than one incident per issue, and less than one-third of 1% of the total membership. In addition, while the incidence of such misrepresentation increased across the three groups, still, only about 2% of the total membership initially committed and then voted *against*. Further, as expected, the incidents ranged so widely across issues that there was no support for the notion that these represented some concerted effort to lull the administration into a false security. Lulling the administration was simply too complex a task for anyone to consider practical. Of course, such very small proportions could reflect something as simple as coding errors, either in White House summaries or data preparation. As a result, these data represent strong evidence supporting the expectation that few will adopt unreasonable positions (expectation 3).

Is Conversion Regular?

As is obvious from Table 1, the Johnson administration's success depended on conversions. Table 4 reports on conver-

Table 4. Patterns of Conversion to *Right* by Types of Member Predispositions

Member Type and Predisposition	n	Actual Occurrences of		
		Initial Hesitancy	Converted	
		n	n	%
$p \leq .2$ (core opponent)	132.7[a]	120.8	27.0	22.4
$.2 < p < .8$ (cross-pressured)	172.5[a]	124.6	51.6	41.4
$p \leq .8$ (core supporter)	128.0[a]	64.2	40.0	62.2

[a] Averages reflect the changing membership of the House during the Congress. Total number of cases is 9,880.

sion among the initially hesitant, again by reference to their predispositions. The question is whether potential bluffers (now identified as concentrated among core supporters) converted wholesale by comparison to the group of more cross-pressured members who also initially responded with hesitancy. In these data, conversion correlates with predispositions. Core opponents convert the least, about 20%, cross-pressured members about 40%, while core supporters (and potential bluffers) convert a bit more than 60%. The rate for core supporters is 50% greater than the rate among the cross-pressured members. Yet despite the fact that these patterns are in the right direction and that the 50% difference is substantial, these data only weakly suggest that core supporters would convert wholesale. Obviously, these members convert more; but they do not *all* convert. There may be a reasonable explanation. During the 90th House, the administration faced a serious conservative challenge on all Great Society legislation. Almost every member of the administration who worked the House has mentioned this fact (e.g., see Sanders 1969). In addition, the average support on initial headcounts had been steadily declining since the high point in the 88th Congress (1964). The administration had lost 47 seats in the off-year election of 1966, mainly among Northern Democrats. Also, the circumstances of the Vietnam War had begun to spill over into domestic legislation.

Several issues presented the administration with unusual legislative problems. Issues like the 1968 tax surcharge or the 1968 railway dispute settlement plagued the administration's supporters—the Northern liberal (often labor-oriented) membership. Some, like Brademas, believed the tax increase should only come in conjunction with tax reform aimed at the "fat cats." Other members believed that the president's desire for a tax increase should not come at the expense of Great Society programs: "Mr. Boland (MA): 'I think those of us who feel strongly on the Great Society programs have to make every effort to send the (tax) bill back to conference and get the conferees to come back with the $4 billion ceiling—the way to do that is to vote down the previous question. That I intend to do.' "[12] On the rail dispute bill, some core supporters wanted the government to protect union rights to strike even at the expense of slowing transport of war material. On both of these issues, as on others, many core supporters were adamant in their initial opposition. For example, in the president's briefing book on the tax bill, the liaison reported on Robert Kastenmeier several times, for instance: "Mr. Kastenmeier: He is firmly opposed to the bill with the $6 billion reduction and would only support it if the cuts came from Vietnam."[13] and "Mr. Kastenmeier: 'This package was put together to please the Republicans and the conservatives—then let the Republicans and the conservatives pass it. Why should liberal Democrats save them?' "[14] The administration also recorded John Dow of New York: "Mr. Dow: Wrong. He will not support a tax increase or an expenditure cut of any kind under any circumstances. He thinks the Vietnam war should be cut."[15]

On the tax surcharge and railway dispute, as on many other issues at that Congress, the administration built its final coalition not by "staying at home" but by compromising with conservatives, as Kastenmeier so hotly noted. In these two situations, the administration compromise left the initially hesitant core supporters unable to hold the administration's toes to the fire. Some of these core supporters responded by carrying through on their threats. Brademas, for example, did not support the administration, nor did Edna Kelly. Undoubtedly, some core supporters carried through because they were genuinely unable to

Table 5. Logit Models of Conversion among Hesitant Core Supporters and the Cross-pressured

Variables	Core Supporters MLE Coefficient	Core Supporters Standard Error	Cross-pressured MLE Coefficient	Cross-pressured Standard Error
Constant	-12.121	4.968*	8.875	3.236***
Constituency instrument	.013	.021	.021	.004***
Institutional context				
LBJ's Gallup approval	-.002	.031	.051	.022*
Democratic leader?	.607	.531	.494	.456
Seniority	-.012	.006	-.006	.004
Total committee value	-.104	.225	-.091	.124
State of the economy				
Inflation rate	.024	.016	-.013	.010
Unemployment rate	.447	.115***	-.291	.065***
Strategic situation				
Initial closeness to majority	-.215	.036***	-.010	.014
(Initial closeness)^2	.178	.032***	-.014	.013
Control for hesitancy: Member's initial response	.203	.044***	.323	.040***
Policy compromise: White House stood firm?	.061	.279	.099	.162
Number of cases	685		1,665	
X^2_{Model}	127.50		268.55	
-2 log (likelihood$_{\text{intercepts}}$)	936.39		2,254.58	
$P_{(-2 \text{ LOG LR})}$	.000		.000	
R^2	.113		.110	
% correct	68.8		67.7	

$^*p \leq .05$.
$^{**}p \leq .01$.
$^{***}p \leq .001$.

reconcile themselves (or perhaps their constituents) to supporting the administration. Others probably saw the situation as an opportunity to reinforce their reputation, since the administration had called their bluff. In any event, the evidence seems to suggest that sometimes core supporters uphold their threats in order to preserve their reputations; and it is this reputational effect that prevents wholesale conversion. Of course, the evidence also suggests that regardless of how firm their objections seem, core supporters convert when their administration needed their support. Despite his strong initial hesitancy, for example, Kastenmeier converted on the tax surcharge, as did Edward Boland, Emilio Daddario, and John Dow.

Is Conversion Strategic?

The previous section suggests that coalition outcomes often can have a strategic as well as a "normal" character. I shall analyze a logit model of conversion designed to capture both the normal and the potentially strategic elements of the coalition-building process. It focuses on the patterns of compromise and conversion among core supporters and suggests a minimum estimate of how many were bargaining and how many were merely bluffing. Table 5 reports the logit model of conversion. In a side-by-side comparison, it reports conversion among hesitant core supporters and hesitant cross-pressured members. The logit specification used the basic diagnostic model plus an intercept dummy describing administration compromise (*standing firm* is the reference value). On these issues the administration either held firm or compromised with conservatives. The model also included a control variable describing the strength of each member's initial hesitancy, using the administration's *right–wrong* scale. This variable controlled for the fact that members with weaker hesitancy might more easily convert. Using predisposition and hesitancy reflects (and controls for) the potential for "normal" conversion. The use of initial hesitancy means that the results reported here probably underestimate the degree of strategic behavior. If members are trying to develop a framework within which they can take advantage without damaging their reputation, they probably consider the strength of their signal as well as its content. They want to be able to signal hesitancy in a way that suggests conversion is possible so that the subsequent conversion will not appear contrived. Given these incentives to hesitate, but gently, there is a correlation between how strongly core supporters hesitate and the bargaining situation.[16] Thus, the use of this hesitancy control reduced estimates of the strategic variables and is a conservative approach to estimating the impact of the bargaining situation on conversion. The adoption of this conservative strategy makes the results reported here stronger evidence of how important the bargaining situation is to the conversion of core supporters. The first expectation was that to the extent there is any systematic variation, the model of conversion would identify as significant the variables describing member predispositions or their initial hesitancy. The second expectation (from expectation 5) was that the model of conversion would identify two other patterns. For potentially bluffing members the model would identify the variables describing the bargaining situation as significant. Among cross-pressured members, the model would identify variables describing members or their relationships with the administration.

The evidence from Table 5 is suggestive. First, the results support the expectation that normal conversion is less important among hesitant core supporters (presumed to be bluffing for the most part) than among the cross-pressured. Among core supporters, the variable describing the member's predisposition is not significant, while only the variable identifying the member's initial hesitancy is. Among the cross-pressured, on the other hand, both variables are significant. Second, as expected, to the extent that there is order to it, conversion among core supporters reflects strategic characteristics, while, by comparison, conversion for cross-pressured members reflects only member characteristics. For example, once the model controls for level of initial hesitancy, the remaining significant variables for core supporters describe the bargaining situation. These include the second-degree equation describing how close to an absolute majority is the initial coalition and the unemployment rate. Meanwhile, member descriptors (e.g., seniority and predisposition) played no role whatsoever in converting core supporters. On the other hand, conversion among cross-pressured members is almost exclusively a matter of the connection between member, constituency, and the president. For example, four of the five variables identified as significant describe the member or the linkage between member and president. The fifth is the unemployment rate.

Combining the results from Tables 2 and 5 provides a picture of how core supporters bargain. It suggests that strategic considerations put an interesting twist on their behavior. Table 2 indicates that given some leeway in the coalition situation (as measured by closeness) core supporters will bargain with the administration. And when their bargaining fails to generate a policy compromise (as on most of the bills here), Table 5 suggests that initial leeway leads to less conversion. When their votes were less important, core supporters could afford to be stubborn for reputation's sake. Both tables, however, suggest that as the administration's initial support nears majority size, core supporters bargain less and convert more readily. The data are interesting because some formal notions of bargaining power suppose that as the initial coalition approaches winning, each additional bargaining member's conversion becomes extraordinarily valuable. Hence, we might expect members to bargain more as the administration's initial coalition efforts come close to winning. Since closeness merely raises the potential value of conversion, however, these models suggest nothing about conversion rates. In order to understand this phenomena, it should be recalled that when information is incomplete, core supporters must walk the fine line between taking advantage and avoiding what would be a policy calamity: without their support *their* administration's proposal could fail. That possibility, plus the fact that policy compromise usually requires coordinated, mass hesitancy, would suggest that as the initial situation narrows to a few votes, it is more likely that any additional hesitant core supporter could cause the administration to give up in defeat and less likely that there are enough others hesitating to force a compromise. In effect, this analysis suggests that members tread a thin line between exercising their potential for power and fearing its consequences: wanting to have bargaining power over the administration without risking affecting the administration's strategic chances for winning. Presented with this complex relationship (including not knowing exactly where things stand with the administration and other supporters), members often will forego their opportunities whenever those opportunities are coincident with high risks. This is just the kind of balance that the earlier discussion presumed would characterize the bargaining relationship between presidents, each member, and their colleagues. This result suggests that future analysis of the detailed member responses might reveal a tendency, as the initial situation becomes close, for members to hesitate by requesting favors rather than requesting policy compromises.

The only other variable accounting for conversion among core supporters also has a strategic connection to Table 2. As the economy worsens, core supporters convert more readily, rallying to the administration in what is a period of weakening influence for presidents. This kind of rally corroborates the results found in Table 2, which suggested that as the economy deteriorated, core supporters were less likely to report hesitancy. In a sense, then, these results suggest that as the president's position weakens, the administration's core foregoes its bargaining advantage and rallies to the administration. On the other hand, cross-pressured members respond to deteriorating economic conditions in ways reflecting the more traditional interpretation of how deteriorating economic performance affects presidential influence.

Finally, there appears to be no evidence to support the notion that compromises converted potentially bluffing members. The coefficient on the compromise variable was insignificant. This result is consistent with my previous findings on compromise covering all counts from the 90th House and including all members (Sullivan 1987). My interpretation stressed the seemingly strategic nature of responses to compromise and the seemingly wholesale conversion of liberal members despite conservative administration compromises. I concluded that presumably "liberals are rallying around the Johnson administration's compromise in order to stem off some worse alternative—in the politician's vernacular, they are getting 'half a loaf' " (Sullivan 1987, 299). That conclusion is certainly consistent with the bargaining behavior identified in the research here.

On the bills analyzed here, the administration never adopted compromises aimed at assuaging its hesitant core supporters; although it did compromise with them on other bills not included in this data. The administration did, however, compromise with its core opponents. For core supporters, these compromised bills cover 52% of the incidents of initial hesitancy. On the other bills, despite core supporter pleas for liberal concessions (similar to those suggested in the report on Brademas quoted earlier), the administration remained firm. Depending on how one reads the demands of hesitant core supporters, it is possible to develop an estimation of what proportion is bluffing. For example, even when the administration cut a deal with conservatives, more than half (53%) of the initially hesitant core supporters converted. These include members like Kastenmeier, Boland, Daddario, and Dow in the tax surcharge example. At the very least, hesitant core supporters who converted despite such conservative compromises represent the absolute minimum estimate of how often members bluff the administration. Afforded that minimum estimate, the quarter of conversions (53% converting of the 52% hesitant) now identified as bluff represents an average of 16.6 votes. That figure is the exact average for the administration's victory margin on its critical votes during the 90th House (217–185). Thus, without those "conversions," the administration would have lost the average critical vote. In addition, on some issues hesitant core supporters were not simply looking to counteract conservative pressure for compromise but were in fact seeking a policy compromise favorable to *their* position. The fact that the administration stood firm and never once gave in represents a negative administration response, which suggests that surely some proportion of the remaining converts probably represent more bluffing hesitancy. If only half of the conversions that took place when the administration remained firm were actually bluffs, as well, the total number of votes involved would be about 27. Given those 27 members, the administration would have lost more than half (55%) of its critical votes in the 90th House. In the end, then, bluffers made up a small but very significant proportion of the administration's core of support, support that was critical to the administration's overall success. For example, on the issues about which it cared a great deal, the administration's batting average for the 90th House would have declined from an impressive 73% to a very disappointing 45%. The administration's overall batting average on counted issues during its full tenure (1963–1968) would have declined from 86% to 54%. That change would have afforded LBJ a success score placing him exactly at the mean success rate for the other post-war administrations for which commitment data are currently available (DDE .596, JFK .615, RMN .512, and GRF .473) instead of his current position as by far the most successful president.

Discussion

While a good deal of this research has been about members' bargaining responses, its implications and the model from which it develops describe aspects of presidential power as well. In order to develop insights about presidential bargaining power properly and apply them to the controversy over bargaining/skill, both the necessary theoretical and empirical agendas are quite substantial. The topics include the theoretical nature of influence (Collier and Sullivan 1988, Sullivan n.d.) and its measurement (Sullivan n.d.), presidential strategies for submitting proposals and signaling (Sullivan 1990), actual member bargaining, the demands members make when they bargain, presidential response strategies, the effect of compromise strategies (Sullivan 1987), and so on. In addition, that agenda would necessarily include developing predisposition models and applying the analysis here to data covering a larger number of Congresses and administrations. Currently, data are available from about six hundred counting efforts drawn from the five administrations Eisenhower through Ford (see Sullivan n.d.). I plan to apply my approach to this larger data set.

The specific empirical analysis reported addressed a central element of "mutual dependence"—the linkage between signaling and choice, or how reputation constrains actual bargaining. It suggests that something limits bargaining so that a specific few members bargain with the administration by withholding support. I shall now propose a simple spatial representation of bargaining. In this representation, bargaining depends on the distribution of opportunity in much the way that Edwards's empirical findings would suggest, that is, elections shape opportunities. The interesting result here is that within this spatial representation it is possible to account for the observed patterns of bargaining only by underscoring the importance of reputation and expectations. Finally, I shall briefly discuss how incomplete information generates a presidential "vantage point."

Two patterns to actual bargaining are important to model. First, of the total membership, few members bluff. Some might have anticipated this pattern, although in the literature on presidential leadership there is no discussion of how to justify such an anticipation. Second, a substantial number of the administration's core supporters feign hesitancy. Clearly, no literature has anticipated this second pattern of bluffing.[17]

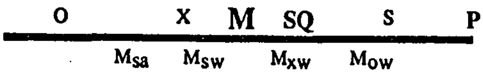
One potential methodological problem, of course, is that the method used here might overestimate misrepresentation, especially among core supporters. The central issue is how to interpret the difference between predicted predispositions and actual behavior: as bias or as politics. There are two general sources of bias: random error and systematic error. There seems little doubt that whatever bias might exist in the model of predispositions, the source is systematic rather than random. For example, the method produced behavioral results that seemed consistent with the expectations advanced, particularly for all $p < .8$.

As for the reasonable possibility that any error may be systematic, here the question is both one of interpretation and methodology. The interpretive question is whether the systematic difference between behavior and predisposition is bias and hence whether the differences are residuals. If so, the problem is misspecification and calls for some better approximation of predisposition. On the other hand, these differences could be strategic and indicative of a political process. The standard practice in studies of mass behavior, of course, is to presume that such systematic differences are residuals in need of corrective measures. When studying elite behavior among professional national politicians, however, there ought to be just as strong a presumption that such differences are not residuals but the tracks of some rational, strategic behavior. This second presumption ought to be particularly strong when the model of predisposition is as powerful as the current model. Indeed, the specification presented here is a better approximation of voting behavior (for example) than standard models of voting (see Weisberg 1978). The fact is that most voting studies do not attempt to focus only on the votes critical to the administration's program. Sometimes the administration keeps the identity of these votes confidential (see Covington 1987b). Sometimes the administration prefers to make its most determined effort in private (see Covington 1987a). In addition to its power, the method adopted here also provides a reliable and valid measure across the range of member predispositions. Thus, it seems reasonable to believe that while every model inevitably excludes some variables, the model presented here does not suffer from such misspecification any more than any modeling effort ever does. Hence, the results of this analysis not only ring true and seem valid but probably are a good approximation (if a first approximation) to what actually happens.[18] The interpretation given these results, therefore, ought to be that the differences between actual behavior and predisposition are probably neither error nor residuals. Instead, the analysis ought to proceed from the presumption that the differences represent the tracks of bargaining behavior between the president and members of Congress. Given that methodological conclusion, it is important to pursue a new model of behavior that would make sense of these strategic deviations from predispositions. That is the objective of the discussion in this final section.

Bargaining Opportunities

The conjecture proposed here is that not everyone has the opportunity to bluff. Take the situation illustrated in Figure 2, which arrays the administration, members, and policy options along a single dimension. The dimension depicts locations from the somewhat extraordinary perspective of the president (extraordinary, that is, by spatial standards, not by those of politicians). This spatial representation approximates Cater's (1964) observation that coalition formation is best understood using the "presidential perspective" on support and opposition. Here, the president's ideal outcome is at P, which also represents one end of the dimension—a perfect predisposition to support the administration. The status quo ante is at SQ, the median of the previous Congress. The president's expectation of the House's decision under normal circumstances is M, the actual "median voter" in the House.[19] Also, the figure locates the median of three different groups. These are the administration's core supporters, the cross-pressured, and core opponents (S, X, and O, respectively—and also as located according to the actual medians for the 90th House). As in the simple game, a group pressures the administration by signaling its intentions. Of course, this model assumes (because the evidence supports it) that members can bring little pressure to bear by committing to vote for the administration. As a result, the analysis presumes that a group must threaten either to vote against the administration or to "take a walk." The administration can anticipate the effect of each threat as a shift in the location of the expected final outcome. The figure indicates these changes as new outcomes M_{ij}, where the subscript denotes the i^{th} group and the j^{th} action. Hence, M_{ow} represents the new outcome the administration expects from its opponents' taking a walk. Since a large proportion of the cross-pressured members and most of the opponents are expected to vote *against*, it is assumed that those two threats (M_{oa} and M_{xa} already are represented by M. Four other potential shifts remain as discinct from M.

Figure 2. Bluffing Potential of Different Factions According to Predispositions



Of the four, two are improvements, from the administration's perspective. If the cross-pressured or core opponents were actually to take a walk, the administration would realize a better outcome than it could have anticipated. Indeed, in this specific situation those located to the left of M cannot pressure the administration because their threats cannot guarantee a worse outcome. Only those to the right can. Those close to M (say, between M and SQ) could pressure the administration except for two elements that probably undermine their potential. First, their absence cannot greatly dislocate the expected outcome. Second, their position on the dimension so close to M will obscure their threat. The administration might mistake their pressure to be M_{xw}. Only the absence of support from the core (either by their voting *against* or taking a walk) could generate outcomes unambiguously moving away from the administration's ideal. Since, in this situation, only the core supporters can misrepresent and make demands on the administration, this model accounts for the two behavior patterns reported earlier. Of course, this is merely a proposal, not a fully articulated model. It does suggest, however, how the distribution and structure of their policy ideals (which, presumably, electoral forces determine) constrain member opportunities. Thus, in the proposed model, opportunities constrain the necessity for presidential bargaining/skill. Hence, such skills may not matter, just as Edwards suggests.

One implication of this representation is that the closer that P comes to M, the more vulnerable to demands and bargaining the president becomes. Thus, a more moderate president (like a Jimmy Carter),[20] whose policy ideal resembles M, faces a situation in which any member's absence drives the outcome away from the administration's preferred policy outcome. Such a president in that kind of situation ends up vulnerable to threats from all quarters as both ends play against the administration's middle. This is not to suggest that these administrations are necessarily less successful than other administrations, since the model only suggests the extent to which bargaining opportunities through threats are available to members. It does suggest, however, why such administrations have a difficult time dealing with the Congress. In effect, this model suggests a paradoxical conclusion. Presidents who can expect to get their ideal policies from the Congress face more troublesome bargaining than those who must *sell* their policies. It further suggests a justification for thinking that bargaining matters, at least for moderate administrations: an administration skilled at bargaining could overcome these kinds of difficulties and still be successful. It also provides a justification for believing that "transformational" leadership (Burns 1978) has some strategic advantages: a president with a vision can narrow the scope of strategic bargaining.

This spatial representation of opportunities seems appealing, it appears to support the Edwards conclusions about bargaining and even suggests some interesting hypotheses about other presidencies. Yet it does not account for all of the observed behavior. It cannot explain why members who *could* bargain nevertheless avoid applying pressure to the administration. Consistent with the reputational hypothesis presented earlier (expectation 2), Table 2 reported that bluffing appears distributed evenly among core supporters. Further examination of Tables 2 and 5 provided evidence that members do not avail themselves of all the opportunities presented them. Rather, they gauged their responses in accordance with how tight the current bargaining situation was, foregoing opportunity whenever *their* administration was in trouble. These two elements are not strictly a part of the opportunities model. They are more strategic in nature.

Also, given the Edwards/opportunity model it is unreasonable to expect that core supporters (or generally any group able to drive the outcome away from the president's ideal) would actually threaten the administration. Such threats made by the administration's core supporters promise to produce a policy that they prefer less than what the administration is proposing. Of course, with complete information, the core supporter's threat would be no threat at all. Were it not for incomplete information, then, members could not utilize their threat potential. And were it not for maintaining a reputation under incomplete information, that is, for periodically carrying out a threat so as to underwrite some future bluff, some members would never carry through on their professed hesitancy.

These considerations simply return the discussion to the notion that the bargaining process matters. In addition to being appealing topics of concern to politicians, reputation and expectations actually play a role in the legislative process mediating between opportunity, threat potential, and choices. A president capable of affecting reputations and expectations can affect the realization of opportunity and block the potential for threat. Hence, the skill to bargain successfully becomes a foundation for presidential power even within the broader context of electorally determined opportunities.

Actually, the limitations of the Edwards/opportunity model in accounting for all that happens has a fairly well established theoretical basis. The preeminent formal result in this area of game theory is that incomplete information generates a very large number of equilibria, or stable bargaining arrangements. An opportunity-driven equilibrium could be one of these, but it need not be the only one. An often-proposed solution to this plethora of equilibria is to identify a "reasonable" set of restrictions on expectations and reputations that would limit thereby the resulting equilibrium set (Fundenberg and Tirole 1983). That recommendation is similar to how a skillful president might limit expectations. Substantive research on what actually happens (like that reported here) is very useful in identifying such reasonable limitations. This search for theoretically meaningful restrictions suggests something useful to presidential scholars as well. While improving the use of information is important (e.g., George 1980), the "proper" exercise of presidential power may require effectively identifying asymmetries and other kinds of restrictions on experiences that shape expectations. In fact, identifying such asymmetries may be one of the ways presidents can find meaning in Neustadt's prescription that presidents should consider their power stakes in the decisions they make. That is to recommend that power stakes should involve the situations, organizations, and behavior patterns that control information and shape expectations.

Information Asymmetries: Exploiting a Presidential Vantage Point

The previous discussion suggests that one advantage an administration can easily develop in bargaining rests on its ability to utilize information. Given their offices, incomplete information is an individual strength for members but a collective weakness. A member's ability to bluff, of course, rests on incomplete information; and some members clearly take advantage of this condition. On the other hand, given the drain on credit, organizing costs, the uncertainty of others' intentions, and the possibility of overreacting to pressure, incomplete information reduces the expected value to members of pressuring the administration. The greater the uncertainty surrounding member positions and strategies, the smaller the expected return to bargaining. An administration can enhance its bargaining by recognizing and taking advantage of the insecurities that members have about their own information, especially as it predicts the collective actions of their colleagues. In particular, it is important for administrations to centralize control over head counting, removing from the congressional party the ability to judge for itself the legislative situation. Indeed, for the most part, that is exactly what the JFK/LBJ OCR accomplished and what the Nixon/Ford/Carter OCRs did not.

Typically, administrations have had the resources necessary to control information, anticipate members' positions, and exploit this uncertainty. Administrations that have utilized their OCRs to gather information about positions (at least the five from Eisenhower through Ford) have gone so far as to perform detailed analyses about their supporters. These studies identify and then analyze critical votes (critical as defined by the administration, not Congressional Quarterly). They also maintain survey data on congressional elections and information about campaign finances in order to monitor the constituency developments that might affect predispositions. (For example, the Johnson administration maintained extensive records on how well members—especially the new members from the 1964 election—were getting on in their constituencies). These kinds of studies improve an administration's picture of its possibilities in Congress. Indeed, one of the reasons LBJ could successfully call their bluffs was his ability to identify just how far he could push the situation and compromise without risking his core supporters. Indeed, because of this skill and information, LBJ managed to maintain sway over the Congress despite ever decreasing levels of initial support (Sullivan n.d.). Clearly, theory development and these kinds of data analyses can broaden current understanding of the president's power to bargain successfully, as well as how members bargain with the president.

Appendix

I shall specify the model of member predispositions and the diagnostic models. The basic member calculus contains two principal components: characteristics of the member (including the constituency and various institutional elements) and characteristics of the bargaining situation. Constituency describes a basic model of preferences, while institutional position and the bargaining situation elaborate that model.

The Model of Predispositions

The model assumes that constituency interests drive member decision making. The link between constituency and choice is a standard element of congressional studies and needs no elaboration. Member preferences reflect the character of their constituencies (see Cox and McCubbins 1986; Cox, McCubbins, and Sullivan 1984; Mayhew 1974; or McCubbins and Sullivan 1984). Presidents can take advantage of this motivation since both the president and members share an electoral fate. Hence, similar constituent bases can unify the two officials behind a common set of long-term political interest, while dissimilar constituencies may set the two apart. These kinds of common outlooks are likely to be long-term, contextual forces acting to identify the member with the policies of the president.

Constituency. Constituency descriptors available from the Census Bureau make up the bulk of the probit estimation used to characterize member predispositions. Table A-2 reports the predisposition model for the *governing the economy* issue cluster. The dependent variable is the probability of supporting the administration $P(Y = 1)$.

Table A-1. A Sample of Counting Efforts in the House, 1967

Leadership Signals	Subject of Counting	Date
Favor	1968 civil rights final passage	1/5
Favor	debt limit increase to $336 billion	2/2
Oppose	recommit on debt limit: exclude certificates	2/2
Favor	Office of Economic Opportunity (OEO) final passage	3/15
Favor	Elementary and Secondary Education Act reauthorization	4/19
Favor	debt limit increase	5/15
Favor	International American Development Bank at $250 million	5/26
Favor	International American Development Bank at $300 million	5/26
Favor	seven-year debt limit extension	6/2
Favor	debt limit increase to $358 billion	6/8
Favor	railway dispute settlement bill	6/14
Favor	District of Columbia reorganization plan	6/19
Favor	Elementary and Secondary Education Act conference report	6/20
Favor	railway dispute conference report	7/16
Favor	rat control bill	7/18
Favor	omnibus safe streets and crime control bill	7/28
Favor	defense appropriations conference report	9/11
Favor	Department of Defense appropriations conference, Byrnes Amendment	9/12
Favor	highway beautification reauthorization	10/18
Favor	rent supplements appropriation	10/20
Favor	participation certificates in Independent Offices Appropriations	10/20
Favor	model cities appropriation	10/20
Oppose	Green Amendment on OEO	11/4
Oppose	recommit motion on OEO	11/4
Favor	OEO conference report	11/15

It is apparent from the summary statistics that this model performs well. For example, it correctly predicts about 83% of the votes. One way to assess just how well this model performs is to compare it to two benchmark models of voting. The first voting model predicts that each member will vote with the majority. A second benchmark takes account of the situations in which the majority votes against the administration. The model of preference is a significant improvement as measured by the Goodman/Kruskal λ (see Weisberg 1978 for an explanation of λ). By comparison, using constituency characteristics improves modeling by nearly 60%: $\lambda_{\text{majority}}$ .586 and $\lambda_{\text{adj. majority}} = .599$.

The Diagnostic Models

To evaluate initial hesitancy and conversion, two diagnostic models elaborate on the electoral model of predispositions. The new diagnostic models start with a constituency instrument based on the predisposition model but avoiding the more complex statement of individual variables. Added to that model are variables describing the member, the bargaining situation, administration compromises or that control for differing degrees of hesitancy.

Describing the Member. This collection of variables includes the constituency instrument and some variables describing characteristics of the member's institutional setting, including Gallup approval ratings of the president. Of course, a member pursues an institutional career, eventually representing a committee or a party as well as a district. Holding a position of institutional authority increases the probability that any particular coalition will reflect the preferences of that member (see Cox, McCubbins, and Sullivan 1984; McCubbins and Sullivan 1984; Sullivan 1984). In utilizing their positions, members employ the organizational mechanisms for accommodation and compromise that are so important to developing coalitions. Over time, the necessity for maintaining these mechanisms redefines the member's preferences. Three variables represent institutional identification within the party hierarchy and the committee system. Membership in the Democratic party hierarchy is a dummy variable (*Yes* is the reference

Table A-2. A Probit Estimate of the Impact of Constituency Variables on Voting on Governing the Economy Issues, 89th Congress (A Model of Predisposition)

Variable Name	MLE Coefficient	Standardized Coefficient	Standard Error	*t*
Constant	-7.770	-.053		
Party identification	3.823	1.809	.033	116.22***
North region	.215	.098	.029	7.52***
South region	-.794	-.329	.052	-15.18***
Percent				
White	-.025	-.004	.144	-.17
Negro	-.875	-.119	.356	-2.46*
Living in standard metropolitan statistical area (SMSA)	2.566	.239	.598	4.30***
Living in SMSA * Party	-.781	-.245	.202	-3.87***
Labor force in military	-1.758	-.085	.355	-4.95***
Labor in private sector	2.708	.223	.443	6.12***
Labor in government work	3.707	.158	.930	3.99***
Labor self-employed	3.758	.225	2.700	1.39
Percent employed in				
Public administration	-1.231	-.028	.623	-1.97*
Manufacturing	-.935	-.097	.153	-6.11***
Agriculture	-2.664	-.194	.268	-9.95***
Retail/wholesale	-1.742	-.043	.582	-2.99**
Transportation	4.597	.070	1.060	4.34***
Construction	.459	.007	1.209	.38
Mining	-.903	-.025	.595	-1.52
Median grade education	-.159	-.205	.018	-8.65***
Median income	.067	.092	.026	2.58**
LBJ's % in district 1964	.044	.545	.002	28.80***
LBJ's % in district 1964 * party	-.021	-.653	.004	-5.00***
Electoral safety	.529	.150	.056	9.43***
Electoral safety * party	-.668	-.297	.464	-1.44
Election cohort	.010	.090	.002	5.82***
Number of weighted cases	16,562.17			
Likelihood ratio statistic	9,332.378			
$P_{(-2\ \text{LOG LR})} =$	.000			
$\approx R^2$	.488[a]			
F	.660			
Percent predicted	.830			

[a] The formula for $\approx R^2$ is from Aldrich and Nelson (1984).

*$p \leq .05$.
**$p \leq .01$.
***$p \leq .0001$.

value). A second variable measures caucus seniority. A third (*total committee value*) measures committee position. The committee value begins with a seniority score calculated by dividing the member's committee rank by the size of the relevant committee caucus. Then, the technique calculates a *seniority value* by adding one to the additive inverse of this seniority score and then multiplying that total by the *Bullock value* for the committee (Bullock 1962). The member's total committee value is the sum of the member's committee seniority values. A final variable in this set links the member to the administration through the president's popularity (Edwards 1980, Neustadt 1960, Rivers and Rose 1985). Gallup polling results act as the operational definition of the president's popularity.

Describing the Bargaining Situation. This collection of variables includes two variables describing the state of the economy and two variables describing the strategic situation. A poor economy undercuts the president's bargaining position as members attempt to insulate themselves from the administration's policies. The unemployment rate for the nation and the change in the consumer price index (the inflation rate) describe the state of the economy. Two variables describe the initial strategic situation. The first is a ratio of *right* responses to the working absolute majority. A second variable is the square of this first variable acting to dampen the main effect.

Controlling for Member Hesitancy. As described earlier, the administration summarized member responses in a variety of ways. On one end of the spectrum were the detailed reports of specific conversations. On the other end were the summary statements ranging from *right* to *wrong* and the grosser distinction between *right* and *hesitancy.* A control variable measures the ordinal relationship captured in the summary statements that ranged from *right* to *wrong*.

Controlling for Administration Compromise. The research assessed the stories in Congressional Quarterly's *Weekly Report* and *Almanac.* It also analyzed White House strategy memorandum to determine the extent to which the administration compromised its position during the coalition-building effort. This variable took on one of three values (−1, 1): liberal compromise, standing firm, or conservative compromise.

Notes

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1. Note, for example, how Jeff Fishel articulates the theoretical appeal of presidential bargaining: "Presidential leadership *is* important. From Richard Neustadt to Paul Light, *the evidence is incontrovertible.* The judicious mix of push and pull, the adroit calculation of resources and costs, the careful ordering of priorities, the selection of senior associates who are politically astute and substantively informed, and the capacity to articulate and sustain a unified vision of where the administration is going, and why, are essential to agenda building and accomplishment" (1985, 188; second emphasis mine).

2. The actions of presidents are exogenous. In making proposals, of course, presidents anticipate

Table 6. Table 1 Revised Using Different Cutoff Values of p

Member Type and Predisposition	n	Predicted As Initially Hesitant[a]		Actual Occurrences of Initial Hesitancy		
		p	n	n	Of Group (%)	Difference[b] (%)
$p \leq .1$ (core opponent)	80.6[c]	.95	76.9	73.2	90.8	-4.8
$.1 < p < .9$ (cross-pressured)	290.9[c]	.52	152.1	205.3	70.6	35.0
$p \geq .9$ (core supporter)	61.6[c]	.07	4.3	30.3	49.3	604.7
$p \leq .25$ (core opponent)	130.1[c]	.92	115.9	117.3	90.2	-1.2
$.25 < p < .75$ (cross-pressured)	130.1[c]	.61	74.2	94.5	72.6	27.3
$p \geq .75$ (core supporter)	143.3[c]	.10	16.6	70.3	49.1	323.7

[a] Based on average probit values.

[b] The "difference" is $(\text{actual} - \text{predicted})_{\text{hesitant}}/\text{predicted}_{\text{hesitant}}$. There is some rounding in these categories.

[c] Averages reflect the changing size of the House membership during the Congress.

congressional reactions, which in turn are dependent on likely presidential propositions, anticipated presidential reactions, and the anticipated reactions of others. The latter, according to Kingdon (1981) and Fenno (1978), may include members of the state delegation, other members in general, or key constituencies. Thus, by comparison with the real world, the basic theory of legislative leadership under examination here is decidedly simple and one-sided. An argument could be presented to the effect that presidents do not have as much flexibility as members do in acting out sophisticated strategies; thus to understand successful presidential leadership, it would first be necessary to develop a theory of how members act.

3. Kingdon (1981, 131) recounts a leadership rundown reflecting the detrimental effect of a bad reputation: "They actually take a list of the members and go down it. . . . ______ will say, 'He just wants me to call so he'll think I'm obligated to him, so don't call him—he'll be all right without it.' "

4. The research focused on the 90th House because it is fairly representative of the postwar presidential experience. The administration was not riding the crest of an election victory. The 1966 midterm elections had eliminated 47 Democratic seats (the 1964 election had created only 38). And LBJ's batting average for the 90th House stood at 80%, only slightly higher than the 1952–1978 average of 74% (Edwards 1980, 19–20).

5. Memo, John Gonella to Barefoot Sanders, 22 May 1968, Personal Papers of Barefoot Sanders, Box 28, "Democratic Opinions on the Tax Bill, 1968," LBJ Library.

6. Memo, Tom Finley to Barefoot Sanders, n.d., Personal Papers of Barefoot Sanders, Box 29, LBJ Library.

7. Memo, Joseph Spilman to Barefoot Sanders, 31 January 1968, Personal Papers of Barefoot Sanders, Box 29, LBJ Library.

8. One peculiarity of these counts is the treatment of members who respond without comment. The administration treats these as different from *undecided.* The analysis here continues this practice by correlating these responses with *wrong* rather than with *undecided.* Using Poole and Rosenthal (1985) ideal points (arrayed from −1.0 to 1.0), an analysis of the spatial location of member coding verifies that the average member coded by the administration as *no comment* is very similar to those members who responded *wrong.* The average locations are *right* (−.274), *leaning right* (−.042), *undecided* (.172), *leaning wrong* (.169), *no comment* (.317), and *wrong* (.388).

9. The *Civil liberties* cluster included jury reform amendments, District of Columbia Charter Act, Voting Rights Act, and so on. *Governing the economy* included Appalachian regional development reauthorization, Equal Opportunity Act, section 14b of Taft-Hartley, clean air, urban renewal, fair labor standards, and so on.

10. Table 6 revises Table 1 under two plausible alternatives to *p* (i.e., 2, .8).

11. Table 2 also tests whether the pattern of hesitancy among core supporters merely was an artifact of the original support probabilities among these members. If so, one core supporter would be hesitant more than another merely because the first had a lower support probability than the second. The coefficient on the constituency instrument derived from the original probit model of preferences (see the Appendix) is insignificant; thus, the relationship does not appear to be spurious. The analysis might have anticipated this result, since the delineation for the core supporters is .8, leaving little room for variance.

12. Memo, DeSautels to Barefoot Sanders, 17 May 1968, Personal Papers of Barefoot Sanders, Box 28, "Democratic Opinions on Tax Bill, 1968," LBJ Library.

13. Memo, Samuel Jones to Barefoot Sanders, 17 May 1968, Personal Papers on Barefoot Sanders, Box 28, "Democratic Opinions on Tax Bill, 1968," LBJ Library.

14. Memo, DeSautels to Barefoot Sanders, 3 June 1968, Personal Papers of Barefoot Sanders, Box 28, "Democratic Opinions on Tax Bill, 1968," LBJ Library.

15. Memo, Asst. Sec. Lashman (HUD) to Barefoot Sanders, 16 May 1968, Personal Papers of Barefoot Sanders, Box 28, "Democratic Opinions on Tax Bill, 1968," LBJ Library.

16. A diagnostic regression assessing which variables affected the strength of core supporter hesitancy confirmed this expectation. It identified as significant all the variables describing the bargaining situation. It identified as significant only the president's popularity from among the constituency-member-administration variables. In addition, the value on the second-degree variable describing the strategic situation suggested that as the situation became tighter, hesitant core supporters ameliorated their hesitancy. Apparently, these bluffing members backed off in order to avoid some calamitous signal.

17. Of course, one possibility to be considered is that the observed patterns may be merely artifacts of modeling predispositions. With a better specification, the model might identify more feigning actors or it might describe as something else altogether the behavior now identified as feigning. While it is impossible to rule out these possibilities, their likelihood is remote. As to the possibility that there are *more* bluffers, the relevant question to ask is, From which group would more bluffing come? There is no rational model to account for bluffing among core opponents. As argued earlier, the administration has a great incentive to assure that such bluffs are extremely costly. That the analysis reported so very few who appeared to bluff their support lends validity to the technique. Among cross-pressured members, there are some good reasons to suspect that the incidence of misrepresentation is very small. For example, since their policy ideals locate them closer to the status quo ante, it is not likely that they could expect to prompt a favorable policy response by feigning positions rather than sincerely stating them. With no rational reason to adopt a feigning strategy aimed at generating a compromise, the only remaining possibility is that these members might feign hesitancy to pressure for some personal favor. It is impossible to rule out this, but a preliminary study of detailed responses suggests that only a small proportion of responses seem to involve requests for favors. So overall, while it remains possible that the lack of strategic actors is an artifact, it seems unlikely. While it is also possible that the model underestimated the incidence of misrepresentation among core supporters, this too seems unlikely. There simply is such large proportion already.

18. There are two specific possibilities worth pursuing in discussing excluded variables. One type of excluded variable describes the economic base of the constituency. While the Census Bureau maintains a Census of Manufacturing, which it regularly includes in its *Congressional District Databook*, it included data reported in 1958. Since the Census Bureau did not update the manufacturing data for the databook supplements following the three waves of court-enforced redistricting in 24 states prior to the 90th House, I excluded them as unreliable. As a result, accurate business data for congressional districts are simply unavailable for this period. To some extent, of course, the demographics of the constituencies reflect some of these kinds of differences. Another possibility is that the model of predisposition might exclude some information captured in the support scores issued by various lobbyists. The model excludes these measures because they are not descriptors of the member's constituency (except where such measures merely are instruments of the constituency variables that determine them). While inclusion of such a score is theoretically inconsistent with the current electoral connection model, results using such an "enhanced" model suggest that there is no difference in how members are *partitioned* on the basis of their predispositions and hence no difference in the analysis of the variance between predicted and actual position taking.

19. The location of medians utilized the probit values describing member predispositions. The location of the median using predisposition was very similar to the location of the median using Poole-Rosenthal mappings.

20. King and Ragsdale (1987, Table 2-17) estimate that during their respective second Congresses, Carter's reconstructed ADA score would have been 73, while LBJ's score would have been 95.

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THE TRIPLE ENTENTE AND THE TRIPLE ALLIANCE 1880–1914: A COLLECTIVE GOODS APPROACH

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The distribution of burdens in alliances may be explained in terms of public and private outputs. A joint product model is applied to the Triple Alliance and Triple Entente, using generalized least squares–auto regressive moving average (GLS–ARMA) techniques and time series data. Results indicate that countries regarded allies' military effort more as complements than as substitutes, though several examples of free-riding behavior are noted. The method used here may provide more accurate estimation of publicness problems than is found in the usual static tests.

Olson and Zeckhauser (1966) modeled the North Atlantic Treaty Organization in the 1960s as sharing the pure public good of strategic nuclear deterrence. Deterrence is a pure public good when its benefits are nonrival and nonexcludable. However, alliances also rely on conventional weapons subject to both partial rivalry and excludability. Consequently, conventional weapons yield benefits of an impure public nature,[1] and may be analyzed with a joint product model in which defense activities are viewed as providing multiple outputs, public *and* private. Empirical tests of the joint product model have been made by Murdoch and Sandler (1984, 1986).

Thies (1987) examined a host of pre–World War II alliances that depended on conventional armaments producing deterrence as well as impure public benefits such as damage-limiting protection in times of conflict. Based on a visual examination of some income and military spending data, Thies (1987) concluded that most of these alliances demonstrated behavior more in keeping with the joint product model for which allies are motivated by private excludable defense benefits. Our purpose is to examine the Triple Alliance and the Triple Entente for the period 1880–1914. We present a joint product model and two-stage generalized least squares estimates of each ally's demand for defense over time.

History and Analyses of Pre-1914 Alliances

The oldest of the alliances was the Dual Alliance between Germany and Austria-Hungary, signed in October 1879, and joined by Italy in May 1882, transforming it into the Triple Alliance.[2] The former remained in force until 1918; the latter ended with Italy's declaration of war on the central powers in 1915. The Dual Alliance pledged assistance if one party was attacked by Russia and at least neutrality if attacked by another power. The Triple

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Alliance was directed primarily against France. It required aid by the other two members if one of them were attacked by France or became involved in a war with two or more other great powers and neutrality if one member initiated war with a nonmember. Italy's membership in the Triple Alliance was never considered firm enough to be reliable in the event of war. Italy and Austria had major territorial disputes. Italy and France settled their differences and signed agreements (December 1900 and November 1902) obliging Italy to remain neutral in the event of France becoming involved in a war with a third party. An Italo-Russian entente in October 1909, with mutual pledges of support in their respective spheres of influence (North Africa and the Balkans), drove another wedge between Italy and its nominal allies in the Triple Alliance.

Russia was associated with the Triple Alliance until 1890. The Alliance of the Three Emperors (June 1881) pledged neutrality in the event of any signatories becoming involved in a war with a fourth party. It was replaced in June 1887 with the Russo-German Reinsurance Treaty, again pledging neutrality in the event of war with a third party, but with two exceptions: German initiation of war against France and Russian initiation of war against Austria. The treaty was allowed to lapse in 1890, following Bismarck's dismissal and despite Russia's desire for renewal.

The Triple Entente was composed of three bilateral accords. The first was a military convention between France and Russia (December 1893), following closely on Germany's refusal to renew the Reinsurance Treaty. Each pledged support for the other in the event of attack by a Triple Alliance power as long as Germany was involved either directly or in a supportive role. The Anglo-French Entente of April 1904 settled some of their considerable colonial differences. The Anglo-Russian Entente completed the prewar pattern of major power alliances, although it nominally did nothing more than defuse mutual tensions in Asia. Neither entente was originally directed against Germany.

The Triple Entente did not (with exception of its Franco-Russian component) entail any specific guarantees of military support, and its component elements were originally mere settlements of extra-European issues. However, once the ententes were signed, "military conversations" followed. In the case of France and Britain, military planning (initiated in 1906 and continued until the outbreak of war) took the form of an agreed division of labor in the event of war with Germany: Britain would concentrate on the war at sea and France on the land war. Anglo-Russian military planning did not begin until June 1914.

There is a voluminous literature on international politics before World War I. Most diplomatic histories, aside from personal memoirs, have focused on the structure of the system and in particular on the reasons why war broke out in 1914 (e.g., Hinsley 1967 and Schroeder 1976). The most-cited factor is the alleged decline in the fluidity of the alliance system and the changing objectives of the great powers. The quantitative literature on nineteenth-century alliances has been mostly devoted to testing propositions derived from the balance-of-power model. Quantitative studies confirm, for example, that the pattern of nineteenth-century alliances was random (Li and Thompson 1978). Despite the intellectual attention devoted to the pre–World War I system, there have been no attempts, aside from Thies (1987), to explicate problems of intra-alliance distribution of defense effort.

Theoretical Model

The joint product model views the arsenal of each ally as providing multiple outputs that may be private (i.e., country-specific, such as protection of colonies), purely public (i.e., nonrival and nonexcludable, such as deterrence against attack), or impurely public between allies (i.e., partially rival and/or partially nonexcludable, such as defense in times of active attack). This public-private distinction concerns nonrivalry and nonexcludability of benefits *between* allies but not necessarily within each ally.

In the case of pre–World War I alliances like the Triple Alliance and the Triple Entente, conventional armaments, in the form of land and navy forces, comprised the arsenals. Such forces are subject to spatial rivalry in the form of thinning (i.e., fewer forces per mile of perimeter, or fewer forces per square mile of area), as more allies rely on a fixed arsenal and its troops. In this case, defense benefits display partial rivalry and cannot be considered to be purely public. Deployment decisions imply excludability for allies furthest from the troops and matériel. Since even conventional forces may deter a potential aggressor from attacking allied nations, some pure public benefits were nevertheless present in these early alliances.

Although the joint product model encompasses the pure public good model, the former may lead to vastly different burden-sharing behavior. Olson and Zeckhauser (1966) demonstrated that the presence of pure public defense benefits implies that (1) the large, rich allies will shoulder the military burdens of the smaller allies; (2) the allocation of resources will be suboptimal with respect to the defense activity; and (3) free riding will occur as an ally reduces its defense commitment in response to an increase in that of the other allies.

The presence of joint products means that the consumption relationship of the jointly derived defense goods plays a role when analyzing free rider behavior. If, for example, the joint products are complementary in consumption (i.e., they enhance one another's marginal valuation), an increase in the other ally's defense output in the form of greater deterrence may augment an ally's own desire to provide defense owing to its need for the private component associated with its own military activity. An ally may therefore respond positively to an increase in the other allies' military activities. Moreover, defense benefits among allies will be shared according to the ratio of private and excludable benefit to total benefits derived from the military activity. Allies that receive relatively more private benefits will spend more on defense; hence, defense burdens among allies will be based in part on private benefit shares rather than economic size.

For alliances relying on conventional armaments, defense benefits are more likely to be complementary since troops and artillery deployed along one border cannot defend a spatially separated border elsewhere in the alliance. In fact, increased fortification of one ally's boundary may induce other allies to follow suit so as not to invite an enemy to direct the assault at the now-relatively-less-fortified perimeter. A negative externality is now at play when allies fortify their borders and territories—that is, the likelihood of an attack elsewhere in the alliance increases. Since the increased military activity of any ally also deters aggression from an adversary, a positive externality is also present.

We now sketch the formal theoretical model. Consider an n-country alliance dependent on conventional armaments. For a representative ally, let x stand for the private defense output, z for the public defense output, and q for the conventional military activity. The ally is indexed with an i, which is suppressed where possible to simplify notation. The joint product relationships in the ith ally are

$$x = f(q) \tag{1}$$

and

$$z = g(q), \tag{2}$$

where $dx/dq = f' > 0$ and $dz/dq = g' > 0$ and both $f(q)$ and $g(q)$ are strictly concave, twice-continuously-differentiable functions. The first derivative of f and g measures the respective marginal productivities of the military activity in providing a private output and alliancewide defense. The quantity of alliancewide defense provided by the *other* allies, denoted by $\tilde{Z}$, affects an ally's decision to build up its own defense. We will refer to the protection benefit derived from an ally's provision (of arsenal and/or troops) as a "spillin." It can be proxied by a physical flow variable (e.g., manpower per period). In total, the allies consume Z units of public protection, where

$$Z = z + \tilde{Z}. \tag{3}$$

If the origin of spillin is important, spillins can be written as

$$\tilde{Z} = \sum_{j \neq i}^{n} g_j(q_j), \tag{4}$$

where $g_j(\bullet)$ relates the defense activity in ally j to the number of units of alliancewide protection added by this ally's activity, namely, z_j. In equation 4, $g_j' > 0$ and $g_j'' < 0$, for all j.

The preferences of a representative, though not necessarily identical, ally are depicted by a well-behaved, strictly concave, nonsatiable utility function,

$$U = U(y, x, Z, T), \tag{5}$$

where y is a private *numéraire* activity and T is a threat variable that could correspond to the sum of the military activity in the opposing alliance.[3] If, however, the opposing nations are viewed differently by a potential adversary, threat would be the *vector* of military manpower of the allies in the threatening alliance. In either case, $\partial U/\partial T = U_T < 0$. An increase in threat lowers utility. Substituting equations 1-4 into equation 5 allows us to express the utility function in terms of activity space:

$$\begin{aligned} U &= U[y, f(q), g(q) + \sum_{j \neq i} g_j(q_j), T] \\ &= U(y, q; q_{-i}, T), \end{aligned} \tag{6}$$

where $q_{-i} = (q_1, \ldots, q_{i-1}, q_{i+1}, \ldots, q_n)$. To derive an ally's demand equation for military activities, a resource or budget constraint,

$$I = y + pq, \tag{7}$$

must limit utility maximization, where I is national income, p is the relative costliness of the military activity, and the price of good y is normalized at a value of one.

Maximizing utility in equation 6 subject to equation 7 and to the constancy of q_{-i} and T yields a set of first-order conditions (FOCs) that can be used to derive an ally's demand for its own military activity. The vector q_{-i} is held constant at the best-response level of the rest of the allies, since a Nash equilibrium underlies our representation of the game structure for the allies' interactions. A Nash equilibrium is achieved when each ally desires an allocation (y^e, q^e) that satisfies its own first-order conditions. Furthermore, at equilibrium *all* allies must be simultaneously satisfying their FOCs. In essence, the best-response level for q_{-i} is the equilibrium vector q^e_{-i} in which each component fulfills the respective ally's FOCs. At equilibrium, the FOCs implicitly define an ally's demand for military activity in terms of the exogenous variables;[4] hence, this demand is

$$q = q(I, p, q_{-i}, T). \tag{8}$$

In the empirical section, equation 8 is estimated for allies in the two pre-World War I alliances. Since we do not have price data on the relative costliness of defense output as compared to private nondefense outputs, we are forced to drop p as an argument. This causes no bias, provided that *relative* prices remain constant over time. In equation 8, the comparative static derivatives $\partial q/\partial I$, $\partial q/\partial q_j$, and $\partial q/\partial T$ prove of interest. Based upon Murdoch and Sandler (1984), these derivatives can be signed a priori in some special cases. The derivative $\partial q/\partial I$ measures the effect of a national income change on an ally's demand for military activity. When defense is a normal good with positive income elasticity, this derivative will exceed zero.

The derivative $\partial q/\partial q_j$ indicates an ally's defense response to an increase in the jth ally's defense activity. Since each ally can receive potential spillins from $n - 1$ allies, there are $n - 1$ responses —that is, $\partial q/\partial q_j$ for $j \neq i$. The sign of these responses are positive when spillins are viewed as complementary to an ally's own defense efforts. If, however, these qs are viewed as substitutes, as in the case of pure public goods, this derivative is more apt to be negative or near zero. Finally, the derivative $\partial q/\partial T$ depicts an ally's defense response to threat. When threat is perceived as significant and increasing, a positive defense response is expected.

The model disaggregates spillins by place of origin (e.g., Austria might respond differently to Germany than to Italy), but can be modified to allow spillins to be perfectly substitutable regardless of place of origin. Replace equation 4 with $\tilde{Z} = h(\tilde{Q})$, where

$$\tilde{Q} = \sum_{j \neq i} q_j$$

and denotes the other allies' aggregate defense activity. Equations 1–3, 5, and 7 remain unchanged. The utility function becomes $U = U(y, q; \tilde{Q}, T)$, and the estimating equation

$$q = q(I, p, \tilde{Q}, T). \qquad (9)$$

The rest of the allies' military activity, $\tilde{Q}$, replaces the vector q_{-i} in equation 8. If threat is also disaggregated by country, T would be a vector of manpower levels rather than the sum of military manpower in the opposing alliance.

Statistical Model

The variables are listed in the Appendix. National income (*INC, where * is a single-letter country designator) data were obtained from Crafts 1983 and Mitchell 1975. Military manpower (*MIL) is reported by the *Statesman's Yearbook* (1879–1914). Threat variables are lagged one period (L*MIL), since a threat must be realized before it is observed; and spillin variables are estimated (*MILE) as the first stage of the two-stage GLS procedure that will be described. Combinations of two or more countries are added to the variable label (e.g., LFRUMIL is the lagged combination of British, Russian, and French military manpower). Changes in alliance composition are detected by changes in *slope* coefficients. A dummy variable (DXY = 1, when alliance between X and Y is in operation) records alliance entry or exit and is multiplied by the estimated military manpower of the country entering or leaving the alliance. Hence, DGR • RE, for example, is a dummy for the Russo-German alliance multiplied by estimated Russian manpower. The coding of the dummy is lagged so that changes in the value of the variable only take effect after one full year of any change in alliance composition.

Estimation of equations 8 and 9 take the form *MIL = f(INC, SPILL, THREAT, SHIFT), where *MIL denotes q, INC depicts GNP or I, SPILL indicates either the allies' aggregate spillins ($\tilde{Q}$) or a vector of allies' spillins (q_{-i}), THREAT reflects the opposing alliance's military proxy, and

SHIFT measures changes in alliance composition. Our estimating equation is

$$
\begin{aligned}
{}^{*}\mathrm{MIL}_t &= \beta_{0t} + \beta_{1t}{}^{*}\mathrm{INC}_t + \beta_{2t}\widehat{\mathrm{SPILL}}_t \\
&\quad + \beta_{3t}\mathrm{THREAT}_{t-1} + \beta_{4t}(\mathrm{D} \bullet \widehat{\mathrm{SPILL}}_t) \\
&\quad + \epsilon_t. \qquad (10)
\end{aligned}
$$

The simultaneity of the country equations was dealt with by utilizing a two-stage least squares process. The SPILL variables were estimated first (this corresponds to the "hats" over SPILL in equation 10), using as regressors the incomes of all alliance partners, the ally's own income, binary (intercept shift) dummies for any alliance in which the country entered or exited during the period (D**), and the exogenous threat of the opposing alliance (LGAIMIL or LFRUMIL). The linear estimates of SPILL were then used for the final estimation of equation 10, using GLS-ARMA (with first-order autocorrelation of residuals), since ordinary least squares estimations resulted in positive autocorrelation.

Results

Results for the Triple Entente and Triple Alliance are presented in Tables 1 and 2, respectively. Since collinearity reduces significance levels, we place some emphasis on coefficients whose values are nearly significant. The positive income coefficient (significant in all cases except Italy) suggests that defense is a normal good. Aggregating the threat and SPILL variables proved unfruitful in some cases, because countries responded to different allies or opponents in different ways. Given the lack of rigidity in the prewar alliance system and the variety of different forms of understandings, this is not surprising. Threat has the expected sign in the majority of cases but is only significant for France. Where the threat sign is negative, it is not statistically significant and is in two cases (Russia reacting to the Triple Alliance and Germany reacting to Russia) limited to situations in which the countries in question had been on friendly terms even though they would eventually be in opposing alliances. Taking the period 1880–1914 as a whole, the positive spillin coefficients suggest little evidence of free riding between countries allied during some part of the period. France and Russia (free riding on one another) are the only exceptions. The slope shift coefficients, however, are predominantly negative (with the exception of France) and typically insignificant, indicating weak evidence of free-riding reactions to alliance formation.

Britain's positive reaction (Table 1) to both major allied groups throughout the period is consistent with the long-standing objective of "splendid isolation," regarding all other major powers as potential enemies. The positive coefficient on spillins (i.e., FRMILE) is nearly significant and suggests complementarity of forces. A larger positive (but insignificant) coefficient is associated with the Triple Alliance. The negative and significant coefficient associated with the alliance slope shift for the Anglo-Russian Entente suggests some substitution of forces once the United Kingdom abandoned splendid isolation. The entente with Russia helped Britain by both relieving military pressure on Britain's Asian empire (Afghanistan) and by diverting German manpower to the East. In the case of France, likely substitution took the form of an agreed specialization of forces in the event of war with Germany. The coefficient on the Anglo-French Entente is, however, insignificant.

The equation for Russia disaggregates spillins by allies. In Table 1, the negative and significant reaction to French military manpower is consistent both with free riding and the two powers' having no serious conflicts. After the Franco-Russian Alliance, a weak degree of complementarity is suggested as seen by the positive but in-

Table 1. GLS-AR(1) Estimates of the Demand for Military Activity for the Triple Entente Allies, 1880–1914

Independent Variables	Dependent Variables		
	UMIL	RMIL	FMIL
Constant	-814.3024[a] (-2.2073)	16,723.9805[a] (5.6685)	3,523.2422[a] (6.1736)
UINC	.0414[a] (2.5260)	—	—
RINC	—	.1745[a] (5.7609)	—
FINC	—	—	.0570[a] (2.1498)
LGAIMIL	.0868 (1.3421)	-.3063 (-1.3761)	.1288[a] (2.4978)
FRMILE	.0461 (1.6667)	—	—
UMILE	—	.6243[a] (1.9143)	.00013 (.0147)
FMILE	—	-1.9916[a] (-4.4751)	—
RMILE	—	—	-.0725[a] (-3.4141)
DFU • FE	-.0115 (-.6261)	—	—
DRU • RE	-.0195[a] (-2.0370)	—	—
DFR • FE	—	.0875 (1.2265)	—
DRU • UE	—	-.2153 (-1.5450)	—
DGR • GE	—	-.0901 (-1.0558)	—
DRI • IE	—	.2485 (1.6479)	—
DFU • UE	—	—	.0360 (1.2822)
DFR • RE	—	—	.0337[a] (2.7896)
DFI • IE	—	—	-.0159 (-.4780)
Rho estimate	.522	.492	.611
Adjusted R^2	.822	.846	.817

Note: t-statistics are in parentheses beneath coefficient estimates.

[a] Significant at the .10 level. This level corresponds to a t-statistic of 1.699 for the United Kingdom, 1.706 for Russia, and 1.703 for France.

significant coefficient of DFR • FE. Free riding is, however, still the net outcome.[5] The positive and significant response to British manpower is indicative of severe rivalry until the entente in 1907, after which the slope shift variable (DRU • UE) indicates some offsetting substitution. The agreement with Italy in 1909 had a positive and nearly significant effect on Russian manpower levels (DRI • IE); as all countries fortified for the approaching war, complementarity among forces may have been fostered. The negative reaction to the Triple Alliance threat is insignificant, as is the reaction to the Russo-German Treaty from 1879 to 1890.

For France, spillins are disaggregated for the United Kingdom and Russia, but

Table 2. GLS-AR(1) Estimates of the Demand for Military Activity for the Triple Alliance Allies, 1880–1914

Independent Variables	Dependent Variables		
	GMIL	AMIL	IMIL
Constant	667.2344 (.4798)	1,440.7637[a] (4.2365)	30.7636 (.0445)
GINC	.0616[a] (2.1376)	—	—
AINC	—	.0393[a] (4.2898)	—
IINC	—	—	.0748 (.7646)
LFUMIL	.1650 (.7752)	—	—
LRMIL	-.0903 (-.9341)	—	—
LFRUMIL	—	-.0340 (-.8402)	.0453 (.4994)
AIMILE	.2755[a] (2.2341)	—	—
GIMILE	—	.0421 (1.6930)	—
GMILE	—	—	.0852 (.9941)
AMILE	—	—	.1463 (.8385)
DGR • RE	-.0052 (-.1790)	—	—
DFI • FE	—	—	-.0441 (-1.1520)
DRI • RE	—	—	-.0075 (-.5951)
Rho estimate	.249	.611	.191
Adjusted R^2	.859	.805	.593

Note: t-statistics are in parentheses beneath coefficient estimates.

[a] Significant at the .10 level. This level corresponds to a t-statistic of 1.699 for Germany, 1.697 for Austria, and 1.701 for Italy.

the Triple Alliance threat is not. A strong positive response (Table 1) to the Triple Alliance reflects the fact that Franco-German enmity pervaded the entire period and dominated French military planning. Unexpectedly, the French response to UK military manpower is insignificant. France's response to Russia (like Russia's to France) is negative. Following the formation of the Franco-Russian alliance after 1893, free riding was attenuated somewhat, since the sign of the slope shift (DFR • RE) is positive of a value less than the coefficient on RMILE.

When France aligned with the United Kingdom and Russia, their forces were complementary against the Triple Alliance threat, as suggested by the signs on DFU • UE and DFR • RE. In the case of the United Kingdom, the entente resulted in agreements that required France to assume the primary responsibility for the land war with Germany. The data confirm that the French army increased markedly in size after 1910, while navy manpower remained static. Given the location of Germany between France and Russia, neither of these allies' forces could substitute for one another, thus explaining the strong positive coefficient on DFR • RE.

For Germany, the threat from Russia is separated from that from France and the United Kingdom because Russia was less of a rival than the latter two entente allies. In Table 2 the sign on LFUMIL is consistent with this rivalry, but the coefficient is insignificant. A negative (but insignificant and near-zero) coefficient is associated with the Russian threat. Similarly, the alliance with Russia until 1890 had no significant impact on German military activity. For most of the period, Russia and Germany were on friendly terms. The positive coefficient on AIMILE indicates that a complementarity existed between the forces of Germany and those of its allies.

Austria's positive and almost significant response to its allies' manpower (GIMILE) suggests that Austrian forces were complementary to those of the other allies (Table 2). This finding is probably due to Italy's being more of an enemy than an ally and to the inability readily to substitute German forces for Austrian in Austria's main theater of interest, the Balkans, owing to transportation difficulties. Austria responds insignificantly to the threat posed by the Triple Entente. Neither Britain nor France presented any direct military danger to Austria.

Given Italy's attempt to foster friendships in both alliances, it is not surprising that the Italian results in Table 2 are inconclusive. None of the coefficients are significant. Even though the results are poor, they are not inconsistent with the theory that makes predictions for "true" allies and enemies. In the Italian case, allies and enemies are impossible to distinguish, especially after the Franco-Italian neutrality agreement of 1900 and 1902.

Conclusion

Little evidence of free riding was found, except for France and Russia. Some free riding occurred in the latter half of the period as the alliances added members and cemented ties prior to World War I. The model used here could be extended to other areas of political inquiry. Many international problems (e.g., global pollution) could be explicated in the same fashion. In the American politics literature, interest groups have yet to be analyzed with the type of time series data that would provide a more accurate assessment of publicness problems than may be found in the usual static or cross-sectional tests.

Appendix: Income, Military Manpower and Alliance Variables

* = country designator, where U = United Kingdom, R = Russia, F = France, G = Germany, A = Austria-Hungary, and I = Italy.

*INC = real GNP in thousands of 1970 U.S. dollars.

*MIL = military manpower in hundreds of men, excluding reserves.

L*MIL = lagged military manpower threat, where LRMIL = Russia, LFUMIL = Anglo-French Entente, LFRUMIL = Triple Entente, and LGAIMIL = Triple Alliance.

*MILE = ally military manpower estimated from income of all alliance members, the lagged threat of the opposing alliance (LGAIMIL or LFRUMIL), and an intercept dummy for any alliance in which country * participated during the period.

D** • *E = military manpower slope shifts resulting from alliance entry or exit: alliance dummy (D** = 1 for entry, 0 for exit) times estimated manpower of entering or exiting party (*E), where DFU • FE, DFU • UE = Anglo-French Entente (0 for 1905 and prior years); DRU • UE, DRU • RE = Anglo-Russian Entente (0 for 1908 and prior years); DGR • RE, DGR • GE = Russo-German alliance (0 for 1892 and subsequent years); DRI • IE, DRI • RE = Russo-Italian agreement (0 for 1910 and prior years); DFI • IE, DFI • FE = Franco-Italian neutrality agreement (0 for 1901 and prior years); and DFR • FE, DFR • RE = Franco-Russian alliance (0 for 1894 and prior years).

Notes

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1. Impurity was mentioned by Olson and Zeckhauser (1966) and formally modeled by Murdoch and Sandler (1984).

2. In a longer unpublished version of this paper (available from the authors) we provide more historical details and examples of pre-1914 alliances.

3. Threat could be modeled as a subtraction from alliancewide protection, Z. Since we do not specifically model the arms race interaction between opposing alliances, our treatment of threat is sufficiently general for the purposes at hand—to derive a reduced-form estimating equation.

4. Sufficiency conditions are satisfied based on the assumptions invoked for the $f(\cdot)$, $g(\cdot)$, $g_i(\cdot)$, and $U(\cdot)$ functions. Thus, the FOCs are both necessary and sufficient for a maximum because the constraints are linear. If, in addition, the associated bordered Hessian determinant is positive definite, equation 8 follows from the FOCs via the implicit function theorem.

5. The net effect is found by adding together −1.9916 and .0875, since the equation is linear.

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STABILITY IN ANARCHIC INTERNATIONAL SYSTEMS

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Can stability emerge solely from the competition and self-interest of sovereign powers existing in a state of anarchy, or does stability depend on restraints from the complex nexus of interdependencies characterizing the contemporary world economy and its associated institutions? We suppose some infinitely divisible resource, that all nation-states are endowed with and maximize and that enables them to overcome adversaries in the event of conflict. We offer a noncooperative, extensive-form model of international conflict without exogenous mechanisms to enforce agreements in order to learn under what conditions balance of power and collective security ensure the sovereignty of all states in anarchic systems. We conclude that there exists at least one world—albeit an abstract one—in which anarchy yields stability.

If we accept the premise that international politics is "a competition of units in the kind of state of nature that knows no restraints other than those which the changing necessities of the game and the shallow conveniences of the players impose" (Hoffmann 1965, vii) and if we also accept that "international relations continue to be a recurring struggle for wealth and power among independent actors in a state of anarchy" (Gilpin 1981, 7), we can interpret much of contemporary theorizing about international politics as concerning whether stability can emerge solely from the competition and self-interest of sovereign states. Can anarchy yield stability, or does stability require restraints from exogenously imposed institutions, from the interdependencies characterizing the contemporary world economy, or, in Riker's (1962) terms, from moral suasion? If "the politics of power turns on the diplomacy by which alliances are made, maintained, and disrupted" (Waltz 1988, 620), does the "invisible hand" of necessity assure us that "in any crisis precipitated by the ambition for preponderance of one state or combination of states, most of the other members of the system will be actuated by the purpose of maintaining equilibrium?" (Claude 1962, 50).

For special cases stability seems unexceptional, as when one country is a hegemon or when there are only two equally powerful states. More generally, though, the difficulty with establishing the existence of this invisible hand is that if cooperation is sustained in anarchy, it must be self-enforcing in the sense that it corresponds to a noncooperative equilibrium in which no one has a unilateral incentive to defect from the agreements essential to stability. And although we know how to establish a noncooperative basis for cooperation in a variety of circumstances,[1] rationalizing realism's argument about stability poses special difficulties. Cooperation and efficiency arise in the repeated prisoner's dilemma, for example, because the punishments allowed by repetition provide an enforcement mechanism whereby all players can avoid mutually distasteful outcomes. In the essential components of the realist view, on the other hand, all outcomes are efficient, since power (a relational concept) is in constant supply. Hence, cooperation can only be directed at implementing or blocking outcomes that are disadvantageous for some and advantageous for others. Anarchic systems, reduced to their basic character, seem susceptible to the instabilities associated with majority rule, and the question remains whether stability is sustainable in *n*-country systems if the primary rule is that countries or alliances of countries with more "power" can defeat those with less.

We shall explore the sources of stability in anarchic systems by building on a model in which countries, represented as unitary actors, are endowed with an infinitely divisible and transferable resource, which they maximize (conditional on being able to ensure their sovereignty) and which measures their ability to overcome adversaries (Niou, Ordeshook, and Rose 1989). However, unlike our earlier analyses, which rely on cooperative game theory, here we conceptualize international politics as an iterative bargaining process in which agreements, if sustained, must be sustained because it is individually rational to do so.

Our analysis begins by differentiating between two forms of stability: system stability and resource stability. *System stability* implies that all countries can ensure their sovereignty—that no country will have its resources reduced to zero. *Resource stability* implies the prediction that no reallocation of resources will occur.

We make this distinction not only to accommodate the possibility that systems can be system-stable without being resource-stable but also to introduce two ideas about *how* stability may be achieved: *balance of power* and *collective security*. Morgenthau (1959) associates balance of power with systems in which "alliances are formed . . . on the basis of what . . . individual nations regard as their separate national interest" (p. 175), which he contrasts with collective security, in which "an attack by any nation upon any member . . . [is treated] as an attack upon all members" (p. 175). Claude (1962, 145) offers an equivalent distinction: "Collective security decrees a set response in support of any victim of aggression; balance of power confirms the freedom of the state to pick and choose." Thus, countries in collective security systems use a different type of strategy than those in a balance-of-power system. Also, goals in the two systems differ. Balance of power seeks to "maintain the equilibrium without destroying the multiplicity of the elements composing it" (p. 157); collective security seeks to preserve the current order, which "is of necessity the status quo" (p. 389). In balance-of-power systems, countries meet threats with anything that works; and their concern is to maintain system stability—whereas stability in collective security systems refers to both system stability and resource stability and arises because threatening states are singled out for punishment.

Although these ideas are subject to a variety of interpretations, we can nevertheless sharpen Morgenthau and Claude's distinction by focusing on two types of equilibria in a game-theoretic model of anarchic systems. The first type, which yields system stability, is sustained by strategies in which, aside from the threats they currently face, players ignore the past. In the search for alliances and counteralliances, players ignore whether potential coalition partners defected from prior agreements. The second type of equilibrium, which models collective security and is intended to yield both system stability and resource stability, is sustained by punishment strategies that target countries that have defected from prior agreements. We seek to determine not only whether each type of equilibria exists (and thus whether stability is possible in anarchy) but also whether stability is easier to sustain with balance-of-power-type strategies or collective-security-type strategies.

Briefly, the analysis we offer rationalizes the conclusion we reach elsewhere using a cooperative model (namely, that system stability can prevail and that the conditions under which this is true require no special number of countries or distribution of resources); but here we look at the self-enforcing character of cooperation and system stability. We offer an extensive-form model of anarchic international systems; explore balance-of-power equilibria for three-country, four-country, and *n*-country systems and establish the conclusion that there exists a balance-of-power equilibrium ensuring the sovereignty of states that are members of minimal winning coalitions; explore collective security equilibria supported by nonstationary strategies in which threatening players are subsequently targeted for retribution; survey the circumstances under which countries that are not members of any minimal winning coalition are eliminated; and suggest when a set of countries might voluntarily relinquish their sovereignty in order to form a new sovereign state.

Our analysis takes no account of geography, resource growth, war costs, uncertainty, and ambiguities in the notion of sovereignty. But because we rationalize many of the conclusions about stability derived from cooperative game theory, our earlier treatments of such matters stand. Our aim, however, is not to formulate a model of international political processes per se but to establish a possibility result that provides a theoretical basis for the intuition that stability is not precluded in anarchy.

An Extensive-Form Model of Conflict

Beginning with some notation, let $(S, \mathbf{r}^\circ)$ be a system, where $S = \{1, 2, \ldots, n\}$ is the set of countries, and $\mathbf{r}^\circ = (r^\circ_1, r^\circ_2, \ldots, r^\circ_n)$ is the current distribution of resources across S. For convenience, we suppose countries are ordered $r^\circ_1 > r^\circ_2 > \ldots > r^\circ_n > 0$, and we let R be the total resources in the system.[2] Winning and losing coalitions are determined by $\mathbf{r}^\circ$, and the sum of resources controlled by the members of the coalition C is $r^\circ(C)$; thus, C is winning if $r^\circ(C) > R/2$, losing if $r^\circ(C) < R/2$, and minimal winning if, for all i in C, $C - \{i\}$ is losing. If $r^\circ_i > R/2$, i is *predominant* (it is winning against all other countries and can incorporate their resources at will); thus, everyone has an incentive to avoid letting some other country become predominant. Countries that control precisely $R/2$ resources are *near-predominant.*

We next describe an extensive-form game modeling a situation in which countries try to increase their resources by making and implementing threats against others and in which threatened countries try to maintain their sovereignty with counteroffers or counterthreats. First, with respect to any distribution $\mathbf{r} = (r_1, r_2, \ldots, r_n)$ different from $\mathbf{r}^\circ$, if $r_i \geq r^\circ_i$ for all $i \in C$, $r_i < r^\circ_i$ for all $i \in S - C$, and $r^\circ(C) > r^\circ(S - C)$, where $\mathbf{r}^\circ$ is the current distribution of resources, then $(\mathbf{r}, C)$ is a *threat* by C against $S - C$. (In the game that follows, many threats and counters may be made in sequence; but the associated coalitions are defined in terms of whatever resource distribution, $\mathbf{r}^\circ$, is the status quo.) Thus, a threat is a proposal to form a coalition that controls a majority of resources for the purpose of expropriating some or all resources from the remaining countries in the system, where some or all countries in the proposed coalition share the expropriated resources. Next, letting T be the set of all threats with respect to $(S, \mathbf{r}^\circ)$,

$$T = \{(\mathbf{r}, C): r^\circ(C) > r^\circ(S - C), C \subset S, \text{ where } i \in C \text{ iff } r_i \geq r^\circ_i\},$$

we let $T_C = \{(\mathbf{r}, C) \in T\}$ denote the set of threats by the coalition C.

The game we offer to model anarchic systems proceeds as follows: A country (say i), randomly chosen by nature, either offers an initial threat $(\mathbf{r}, C)$ or passes. If i passes, nature selects another country. If i does not pass, the members of $C - \{i\}$ simultaneously decide whether to participate in the threat. If they unanimously accept this threat, it becomes the currrent threat, and threatened countries must respond. Responses are of two types. First, each threatened country, taken in sequence, can offer a counterthreat, which is a new threat and, if unanimously accepted by the newly proposed coalition C', cancels the original threat and becomes the new current threat. The second type of response is a proposal by one or more threatened countries to surrender resources to one or more members of the originally threatening coalition. If a transfer is accepted by the countries involved, it determines a new status quo; and the game proceeds as before. More formally, we define the game Γ thus:

1. Nature randomly selects $i \in S$.
2. i threatens $(\mathbf{r}, C) \in T_C$, $i \in C$; or passes. If i passes, we return to step 1. If i threatens and if $r_j > r^\circ_j$, j is an active member of C; whereas j is passive if $r_j = r^\circ_j$.[3]
3. Members of $C - \{i\}$ *simultaneously* choose between approving or rejecting $(\mathbf{r}, C)$. If unanimously approved, $(\mathbf{r}, C)$ becomes the *current threat*; otherwise, return to step 1.
4. With $(\mathbf{r}, C)$ the current threat, nature randomly orders the members of $S - C$; we denote this order by $O = (m, l, \ldots, k)$.
5. With m in $S - C$ offering the first *counter*, a counter takes one of two forms: a new threat, $(\mathbf{r}', C') \in T_{C'}$, $m \in C'$, or a resource transfer from $S - C$ to one or more members of C. We denote those party to the transfer by C'.
6. Members of $C' - \{m\}$ *simultaneously* choose between approving or rejecting m's counter. If approval is unanimous, $(\mathbf{r}', C')$ becomes the new current threat, and we return to step 4. If one or more members of $C' - \{m\}$ reject the counter, we select the next country in O and return to step 5. For counters that are resource transfers, unanimous acceptance renders the transfer the new status quo and we return to step 1.
7. If the last country in O's counter is rejected, the current threat's distribution, $\mathbf{r}$, becomes the status quo (set $\mathbf{r}^\circ = \mathbf{r}$), j is eliminated if $r_j = 0$, and we return to step 1.

This sequence of moves is, of course, a gross simplification of even a simple view of international processes. For example, i's partners in C can accept or reject the proposed threat $(\mathbf{r}, C)$, but they cannot offer modifications. So Γ ignores the negotiations over the distribution of spoils that might precede alliance formation and conflict. Also, Γ offers a highly structured interpretation of what we mean by anarchy. Nevertheless, Γ does model a system in which power is the sole determinant of winning and losing, threats and counters are the mechanisms whereby countries secure resources and ensure their sovereignty, countries join coalitions because it is in their individual interest to do so, and no exogenous constraints ameliorate conflict. Moreover, because we allow countries to consider the consequences of their actions fully, our model accommodates Kaplan's (1979,

70) assertion that any analysis of international stability must take account of the possibility that "the weakest player, by joining a nearly predominant strong player, only creates a condition in which he will be the next victim."

With respect to how we can begin to analyze this game, we must first recognize that Γ's principal analytic feature is that it is a *recursive* game, which allows for the possibility that threats and counters, as well as reallocations of resources, continue in sequence forever. Despite the possibility of endless play, however, the standard treatment of such a game is to pretend that it is finite by supposing that we know the consequences of all branches of its extensive form.[4] As Wagner (1986, 551) describes the task: "The basic question that concerns us is whether states will act so as to eliminate other states. If one state is eliminated from a four-actor game, for example, the result is to precipitate a three-actor game. If a value can be assigned to such a subgame for each player, it is possible to determine whether any players have an incentive to eliminate other players."[5] After postulating these consequences, we then deduce an equilibrium by finding strategies that are self-enforcing in the sense that no player has an incentive to defect unilaterally to any choice not dictated by that player's strategy. Thus, our equilibrium is necessarily subgame-perfect. However, an equilibrium requires that the postulated consequences be consistent in the sense that they become "self-fulfilling prophesies"—that they be produced by the subgame-perfect choices they imply.

To pursue this type of analysis, however, we must first make some assumptions about the types of strategies that we will consider as potential sources of equilibria. Specifically, the possibility of infinite play admits the possibility of very complicated strategies. However, in most instances these strategies strain credulity, because they are so complex or because they entail counterintuitive behavior (e.g., a player punishes another for not punishing him). Fortunately, the literature on balance of power and collective security point us in the direction of some specific intuitively plausible strategies.

To model balance of power, we suppose that strategies are *stationary*—that countries pursue the same strategy whenever the status quo distributions are identical and they encounter the same threat and thereby ignore who made a threat or who agreed to participate in a threat. To model collective security, we look at simple *punishment strategies*—strategies that punish those who try to upset the status quo either by making a threat or by agreeing to participate in a threat. We shall focus first on stationary strategies, postponing discussion of equilibria supported by punishment strategies because the analysis of stationary equilibria will help us analyze punishment equilibria.

With respect, then, to stationary strategies, notice that each subgame of Γ that begins after a threat is proposed and accepted can be characterized by a specification of this threat plus a description of the status quo resource distribution. If we simplify matters by suppressing any notation for $\mathbf{r}^\circ$ and if, in accordance with the assumption that strategies are stationary, we suppose that a country's response to $(\mathbf{r}, C)$ is invariant with who makes this threat and with the histories associated with other players, we can let Γ_r denote the game that follows the acceptance of the threat $(\mathbf{r}, C)$. If country i associates the value $v_i(\Gamma_r)$ with playing the subgame Γ_r, the notation $\mathbf{v}(\Gamma_r) = (v_1(\Gamma_r), \ldots, v_n(\Gamma_r))$—the *continuation value* of Γ_r—summarizes what the countries believe follows from the approval of $(\mathbf{r}, C)$. Thus, $v_i(\Gamma_r)$ determines i's preference for proposing, accepting, or rejecting $(\mathbf{r}, C)$. Once values for all threats are specified, we can assume that the acceptance of a threat or counter is a terminal node with its continuation value as the "final outcome."

We then analyze Γ as though it were a finite extensive-form game of complete information and we deduce subgame-perfect equilibrium strategies by working backward from the terminal nodes just as we treat finite agendas in majority voting games: we deduce what each country ought to do any time it must choose a threat, a counter, or accepting or rejecting a threat or counter.

The problem here is that we have merely pretended to know continuation values. Nevertheless, we can define an equilibrium as a set of continuation values (one for each threat [subgame]) and a set of strategies for each country such that these values and strategies are consistent. Thus, in equilibrium, the choices the continuation values imply (the strategies that are a subgame-perfect equilibrium, given the continuation values) must, in turn, imply those continuation values.

The preceding argument "works," however, only if Γ has terminal nodes. First, we initially interpret nodes in which a threat is implemented as terminal, although we do this with the understanding that such nodes are merely the starting point for a new round of conflict with a new status quo. Any evaluation that countries place on such reallocations, then, depends on what follows in the new round. There is a second type of terminal point, however, which arises whenever one country secures $R/2$ resources, owing to the assumption that if $r°_i = R/2$ and the threat $(\mathbf{r}, C)$ is implemented, with $i \in C$, then even if $r_i = r°_i$, i can take advantage of the conflict to become predominant. So systems with a near-predominant country are "frozen." No additional threats are made because the remaining countries will block any threat. We can rationalize this assumption by supposing that $r°_i$ measures resource proportion and that the conflict implicit in a threat's implementation destroys some of the active antagonists' resources. For example, if $\mathbf{r}° = (50, 33, 17)$ and the threat $((50, 50, 0), \{1, 2\})$ is implemented with some of 2's and 3's resources destroyed in the process, 1's relative share increases and it becomes predominant.

Terminal nodes in which a country becomes near-predominant can be reached if an appropriate threat is implemented, but they can also be reached if countries transfer some share of their resources to another country. We have not specified the circumstances under which transfers are proposed, but the incentives for accepting them also derive from the idea that conflict is costly. Without modeling war costs directly, we can make allowances for such costs with the assumption (Niou and Ordeshook 1989) that if i can become near-predominant by implementing a threat or by having countries transfer enough resources to render i near-predominant, i prefers the transfer. In confronting the threat $(\mathbf{r}, C)$, suppose that the members of $S - C$ find it in their interest to "freeze" the system. Defining max[C] as the largest member of C with respect to the current, status quo, resource distribution (formally, $\max[C] = \operatorname{argmax}[r°_i]_{i \in C}$), then, if $r°(S - C) + r°_{\max[C]} > R/2$ (if threatened members can form a winning coalition with a threatening country), the system is frozen if $S - C$ offers to render max[C] near-predominant. Clearly, if $S - C$ prefers freezing the system, it should transfer to max[C], since this choice minimizes the resources that $S - C$ must surrender. And max[C] accepts the offer: because attempts to secure more than $R/2$ will be blocked, securing $R/2$ by transfer is max[C]'s most preferred feasible outcome.

The circumstances under which countries propose such transfers is an issue we cannot yet address; but we must consider a second type of transfer—by $S - C$ to members of $C - \{\max[C]\}$—say C'—such that $r'_j \geq r_j$ for all $j \in C'$, where r'_j is the proposed posttransfer resources of *j*. This issue of nonterminal transfers raises an additional question about payoffs. In the case of terminal nodes (whenever some country is near-predominant), it is natural to assume that $u_i(\mathbf{r}) = r_i$. A problem arises, however, with respect to *i*'s evaluation of $\mathbf{r}$ when $\mathbf{r}$ is a nonterminal distribution occasioned by a resource transfer. One possibility is that we could posit continuation values for the subgame beginning with $\mathbf{r}$ as a new status quo point just as we posit them for threats. Rather than concern ourselves, however, with complex expected-value calculations, we assume that countries are risk-averse in the sense that if $\mathrm{R}(r)$ is the set of terminal and nonterminal resource distributions that might be reached from $\mathbf{r}$, $u_i(\mathbf{r}) = \min[R_i(\mathbf{r})]$. This assumption prunes nonterminal transfers from our game's extensive form in two ways. First, if *i* must choose between r_i with certainty (as when another country transfers to a third party that ends the game) versus playing a new game with r'_i resources, with $r'_i > r'_i$, *i* prefers the certainty of r_i, since, in equilibrium, *i* may be threatened subsequently with the necessity for transferring resources itself to a level below r_i. Second, if *i* is threatened and if in countering this threat *i* must choose between retaining r_i after it transfers to end the game and retaining r'_i, $r'_i > r_i$ by transferring a smaller amount so as to attract someone away from the threatening coalition, *i* prefers the terminating transfer.[6]

A Three-Country Example

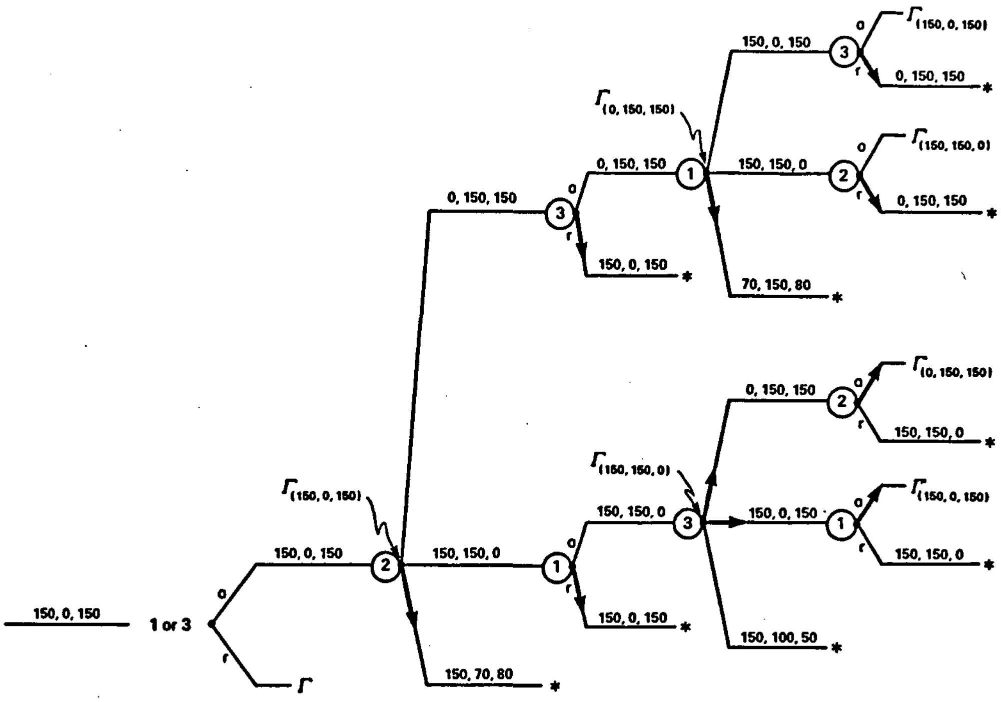
To illustrate the preceding development with the simplest possibility, let $(S, \mathbf{r}^\circ) = (\{1, 2, 3\}, (120, 100, 80))$. Suppose nature chooses country 1 to move first, and consider what happens if 1 threatens 2 with $((150, 0, 150), \{1, 3\})$. Temporarily limiting our discussion to threats such as this one, which eliminates a country (thereby ignoring threats by only one country against another) and to terminal transfers, consider Figure 1's representation of the situation (* denotes a terminal node). After 1 proposes the threat, 3 must decide whether to accept or reject it. If 3 accepts, 2 must counter with either a proposal to coalesce with 1 or with 3 to divide *R* or an offer to transfer to 1 (which 1 is certain to accept since this is the best possible payoff for 1, given that 2 and 3 will never allow an outcome that gives 1 more). Country 2 need not consider a transfer to 3, since transferring to 1 in order to freeze the system entails the loss of fewer resources. Depending, then, on which counterthreat 2 chooses, either 1 or 3 must decide whether to accept or reject. In the event of a rejection, the threat is implemented and 2 is eliminated. In the event of an acceptance, the counter becomes the new current threat, and the subgame that follows is denoted by Γ with an appropriate subscript. Figure 1 portrays the next step in this process, with either 1 or 3 offering a counter.

To analyze this game further we must posit continuation values for the threats that eliminate a country:

$$\mathbf{v}(\Gamma_{(150,0,150)}) = (150, 70, 80)$$
$$\mathbf{v}(\Gamma_{(0,150,150)}) = (70, 150, 80)$$
$$\mathbf{v}(\Gamma_{(150,150,0)}) = \mathbf{v}(\Gamma_{(150,0,150)})/2 + \mathbf{v}(\Gamma_{(0,150,150)})/2 = (110, 110, 80)$$

To deduce subgame-perfect equilibrium choices for the extensive form in Figure 1, we begin at the top right of this figure. Notice first that country 3 prefers (0, 150, 150) over $\Gamma_{(150,0,150)}$, since $v_3(\Gamma_{(150,0,150)}) = 80$. We indicate this preference by an arrow. Similarly, 2 rejects $\Gamma_{(150,150,0)}$ since $v_2(\Gamma_{(150,150,0)})$ corresponds to a lottery between 150 and 70, whereas rejection gives 2 a payoff of 150. Thus, if 2 counters the initial threat of (150, 0, 150) with (0, 150, 150) and if 3 accepts, 1 prefers to transfer resources to 2, because to choose

Figure 1. Subgame Following the Threat of (150, 0, 150)


150, 0, 150
1 or 3
o
r
Γ
150, 0, 150
2
Γ(150, 0, 150)
0, 150, 150
150, 150, 0
150, 70, 80
*
3
o
r
0, 150, 150
150, 0, 150
*
1
Γ(0, 150, 150)
150, 0, 150
150, 150, 0
70, 150, 80
*
3
o
r
Γ(150, 0, 150)
0, 150, 150
*
2
o
r
Γ(150, 150, 0)
0, 150, 150
*
1
o
r
150, 150, 0
150, 0, 150
*
3
Γ(150, 150, 0)
0, 150, 150
150, 0, 150
150, 100, 50
*
2
o
r
Γ(0, 150, 150)
150, 150, 0
*
1
o
r
Γ(150, 0, 150)
150, 150, 0
*

otherwise leads to 1's elimination. (We need not consider any other type of transfer: transfers giving less than 150 are rejected, since rejection implements the threat, and a transfer that renders 3 near-predominant is more costly than a transfer to 2.) Notice, now, that 1's choice of a transfer implies that $v(\Gamma_{(0,150,150)}) = (70, 150, 80)$, which corresponds to our initial supposition. This fact means that 3 would reject 2's counterproposal to threaten 1.

The lower right part of Figure 1 corresponds to country 2's countering with a threat against 3. If 1 accepts this counter, (150, 150, 0) becomes the current threat. At this point 3 is indifferent between countering with (0, 150, 150) and (150, 0, 150), so if 3 chooses with equal probability whenever it is indifferent, $v(\Gamma_{(150,150,0)})$ is consistent with our initial supposition about continuation values. But this means that if 1 accepts 2's counteroffer, it secures a lottery between 150 and 70; whereas if it rejects, it gets 150. So 1 rejects 2's offer. Since 3 rejects a counter of (0, 150, 150) and since 1 rejects a counter of (150, 150, 0), 2's sole course of action when confronting the threat of (150, 0, 150) is to transfer resources to 1 so as to render 1 near-predominant. Thus, as initially conjectured, $v(\Gamma_{(150,0,150)}) = (150, 70, 80)$.

An equivalent analysis establishes the consistency of the conjectured continuation values for (150, 150, 0) and (0, 150, 150) as initial threats—the sole difference in the case of (150, 150, 0) being that country 3, in responding to the initial threat, is indifferent between proposing a coalition with 1 or 2; thus, our earlier specification of $v(\Gamma_{(150,150,0)})$ assumes that 3 chooses one or the other with equal probability. What remains is a specification of $v(\Gamma)$ and an equilibrium identifying choices and responses of countries. Suppose first that $v(\Gamma) = (120, 100, 80)$—that is, that both system stability and resource stability prevail, which we presume arises because no country makes or accepts an initial threat. This supposition is sustainable, in fact, if we characterize equilibrium strategies by saying that a country does not initiate or agree to a threat unless it gains resources. Since the $v(\Gamma_r)$'s imply that countries 1 and 2 can each gain from an initial threat (whereas 3 can neither gain nor lose), 3 has no positive incentive to participate in a threat. Thus, the only threat that might be approved is (150, 150, 0). However, 1 and 2 are essentially playing a zero-sum game; thus, depending on the probability that 3 chooses one action or another when it is indifferent, neither 1 nor 2 has an incentive to threaten or approve of (150, 150, 0). Indeed, 3 can counter with a threat that requires the originally threatening country to transfer resources to 3's partner in the counter. Hence, three-country systems can be both system-stable and resource-stable.

There is, however, another stationary equilibrium that fails to yield resource stability. This equilibrium, characterized by the strategy of accepting all initial threats if they promise no loss, otherwise rejecting, sustains $v(\Gamma) = (150, 70, 80)/2 + (70, 150, 80)/2 = (110, 110, 80)$. Although country 3 cannot gain resources if it abides by such a strategy (the continuation values for all threats remain as before), 3 has no positive incentive to defect to another strategy. Hence, there exists a stationary equilibrium in which three-country systems are resource-unstable. However, regardless of which stationary equilibrium prevails, this three-country system is system-stable; and since all three-country systems without a predominant or near-predominant player are equivalent, we see that stability of the sort envisioned by realist theorists is possible in anarchic systems.

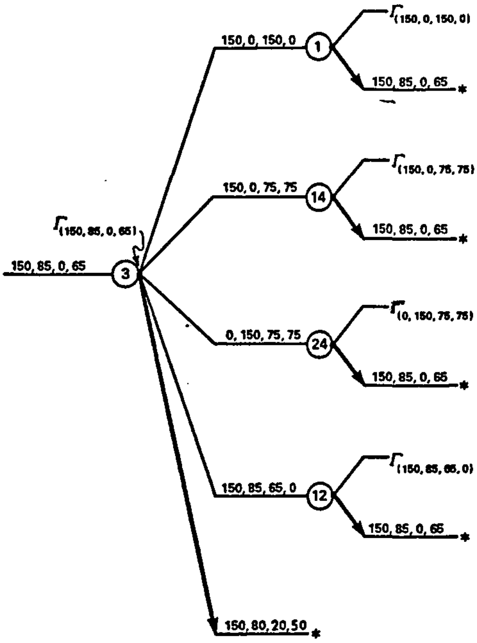
Four-Country Examples

Three-country systems differ from larger ones in that if no country is predominant, all are *essential*—that is, all are members of a minimal winning coalition. For a system with an *inessential* country (a country that is not a member of a minimal winning coalition) we must consider larger systems. To see that this distinction is important, let $\mathbf{r}^{\circ} = (100, 95, 75, 30)$—so that 4 is inessential—and let the presumed equilibrium be characterized by the statement "Countries make or accept threats if they do not lose from doing so." Now let 3 threaten (100, 95, 105, 0). Country 4 must coalesce with 1 or 2 because if 3 cannot gain resources, 3 will reject any offer, thereby implementing (100, 95, 105, 0). But if 1 or 2 accepts 4's offer, each is vulnerable to counters by 3 in the form (150, 0, 150, 0) or (0, 150, 150, 0).[7] Thus, 1 and 2 reject 4's overtures, and 4 succumbs to the initial threat and is eliminated.[8] Thus, the system ({1, 2, 3, 4}, (100, 95, 75, 30)) is not system-stable. On the other hand, if 4 is essential, as it is with the distribution $\mathbf{r}^{\circ} = (110, 80, 60, 50)$, every country can buy stability and (as with three countries) none is eliminated. Nevertheless, we can use this example to illustrate the role of certain assumptions.

Limiting discussion again to specific threats and counters (which, we show later, play a special role), consider the following threats and continuation values (where $a < 150$, $b < 150$, $c < 60$, and $d < 50$ and the values of a, b, c, and d depend on the threat in question):

$$v(\Gamma_{(150,150,0,0)}) = (a, b, 60, 50)$$
$$v(\Gamma_{(150,0,0,150)}) = (a, b, 60, d)$$
$$v(\Gamma_{(150,85,0,65)}) = (150, 80, 20, 50)$$
$$v(\Gamma_{(150,0,150,0)}) = (a, b, c, 50)$$

Figure 2. Subgame Following the Threat of (150, 85, 0, 65)



$$\mathbf{v}(\Gamma_{(150,85,65,0)}) = (150,\ 80,\ 60,\ 10)$$
$$\mathbf{v}(\Gamma_{(150,0,75,75)}) = (150,\ 40,\ 60,\ 50)$$
$$\mathbf{v}(\Gamma_{(0,150,75,75)}) = (40,\ 150,\ 60,\ 50)$$

We begin with the possibility that in confronting any of these threats, instead of countering with a threat from this list, a country in $S - C$ proposes a resource transfer to max[C]. Notice that if two countries are simultaneously threatened and the first country to counter proposes a transfer to freeze the system, it cannot require that its partner in $S - C$ bear the greatest burden. If it proposes such a transfer, its partner can reject and propose instead that the first country bear nearly all of the burden; and at this stage that country has little choice but to comply, since choosing otherwise implements the original threat. Further, if any other counter by the first country is rejected, the second can again propose, if it is in its interest to do so, that the first country bear the greatest burden. We can imagine more complex possibilities for threatened countries; but the details of who transfers to whom has little bearing on our general conclusions, and our representation implies that whatever country responds first must bear the greatest burden in the event of a transfer by $S - C$ to max[C].

Figure 2 shows the extensive form when (150, 85, 0, 65) is the current threat. Country 3 has five alternative actions: four counterthreats and a resource transfer to 1. Briefly:

1. If 3 proposes (150, 0, 150, 0) as a counter, 1 rejects, since, by assumption, $v_1(\Gamma_{(150,0,150,0)}) = a < 150$ and rejection implements a threat that gives 150 to 1.
2. If 3 proposes (150, 0, 75, 75), country 4 rejects, since $v_4(\Gamma_{(150,0,75,75)}) = 50 < 65$.
3. If 3 proposes (0, 150, 75, 75), 4 rejects for the same reason.
4. If 3 proposes (150, 85, 65, 0), 2 rejects, since $v_2(\Gamma_{(150,85,65,0)}) = 80 < 85$.

Hence, the only alternative available to 3 is to transfer resources to 1, in which case, as conjectured, $\mathbf{v}(\Gamma_{(150,85,65,0)}) = (150, 80, 20, 50)$. An equivalent analysis holds for the threats corresponding to the distributions (150, 85, 65, 0), (150, 0, 75, 75), and (0, 150, 75, 75).

The situation is more complicated if two countries are threatened, as when {1, 2} threatens {3, 4} with (150, 150, 0, 0). Figure 3 shows the part of the extensive form that pertains after such a threat is accepted and after nature selects 3 to offer the first counter (the situation is symmetric if 4 counters first). As before, 3 has four counterthreats; but their rejection, rather than leading to the implementation of the threat, gives 4 an opportunity to offer a counter. Since 4's options are independent of 3's choice, Figure 3 portrays only one instance of 4's decision.

Working backward on the extensive form, so as to identify subgame-perfect equilibrium strategies, and looking at 4's decision, we see that

1. if 4 offers (150, 0, 0, 150), 1 is certain to reject, since $v_1(\Gamma_{(150,0,0,150)}) = a < 150$
2. if 4 offers (150, 85, 0, 65), 2 rejects, since $v_2(\Gamma_{(150,85,0,65)}) = 80 < 150$
3. if 4 offers (150, 0, 75, 75), {1, 3} accepts
4. if 4 offers (0, 150, 75, 75), {2, 3} accepts

Since 4 is indifferent between these last two possibilities, we suppose that 4 chooses one or the other with equal probability. Thus, there is no reason for 4 to consider its last option—a transfer to 1. Looking now at 3's choice in light of Figure 3,

1. because rejection of any counter by 3 yields a lottery between $\mathrm{v}(\Gamma_{(150,0,75,75)}) = (150, 40, 60, 50)$ and $\mathrm{v}(\Gamma_{(0,150,75,75)}) = (40, 150, 60, 50)$, if 1 prefers v_1(150, 0, 150, 0) to a lottery between 150 and 40, 3 does not counter with (150, 0, 150, 0)—$v_3(\Gamma_{(150,0,150,0)}) = c < 60$ and, as we see shortly, 3 has better choices
2. if 1 prefers the lottery, the counter (150, 0, 150, 0) is equivalent to the counter (150, 85, 65, 0), since 2 rejects in favor of the lottery.
3. The counters (150, 0, 75, 75) and (0, 150, 75, 75)—both of which yield 3 a payoff of 60—are accepted by {1, 4} and {2, 4}, respectively, which precludes the necessity of considering a transfer to 1.

Now, however, we introduce an assumption that does not affect our conclusions here, but simplifies proofs later: suppose that $i \in S - C$ chooses ($\mathbf{r}'$, C') as a counter to ($\mathbf{r}$, C) such that $S - C \subseteq C'$ whenever i is otherwise indifferent. Later, we state this assumption formally; but its rationale is that the threat against $S - C$ makes the formation of $S - C$ less costly. (Also, if i is indifferent, such a choice can characterize an equilibrium strategy, since i has no positive incentive to choose differently.) Presently, this assumption implies that 3 counters with (150, 0, 75, 75) or (0, 150, 75, 75). And, since an identical argument holds if 4 counters first, $\mathrm{v}(\Gamma_{(150,150,0,0)})$ is a lottery between (150, 40, 60, 50) and (40, 150, 60, 50), as originally asserted.

Finally, to see why a threat of (150, 0, 150, 0) does not lead with certainty to 1's near-predominance (see Figure 4), suppose that 2 counters first. If 2's counter is rejected, then, given the presumed continuation values, 4 can propose (150, 85, 0, 65), which yields (150, 80, 20, 50), which will be accepted by all partners and ensures that 4 loses no resources. Faced with this consequence of a rejection, we can look at 2's four possible counterthreats to the original initial threat:

1. If 2 proposes (150, 150, 0, 0), 1 rejects and secures (150, 80, 20, 50).
2. If 2 counters with (150, 85, 65, 0), 1 is indifferent (3 accepts); but regardless of how 1 chooses, 2 gets 80.
3. If 2 counters with (150, 85, 0, 65), 1 is again indifferent and 2 gets 80.
4. If 2 counters with (0, 150, 75, 75), 4 is indifferent (3 accepts), giving 2 a lottery between 150 and 80.

Thus, (0, 150, 75, 75) is 2's best response. And since nature can choose either 2 or 4, this argument establishes that $v_1(\Gamma_{(150,0,150,0)}) < 150$ and $v_3(\Gamma_{(150,0,150,0)}) < 60$. If nature selects 4 for the first counter, Figure 4 shows that if 4's counter is rejected, 2 must propose (150, 85, 0, 65), because (150, 85, 65, 0) and (0, 150, 75, 75) is rejected by 3, whereas (150, 150, 0, 0) is rejected by 1. This implies that 1 rejects (150, 0, 0, 150); 1 is indifferent between accepting and rejecting (150, 0 75, 75) (3 accepts); countries 2 and 3 accept (0, 150, 75, 75); and 1 and 2 are indifferent between accepting and rejecting (150, 85, 0, 65). Applying the same assumption as before (that 4 chooses a

Figure 3. Subgame Following the Threat of (150, 150, 0, 0)

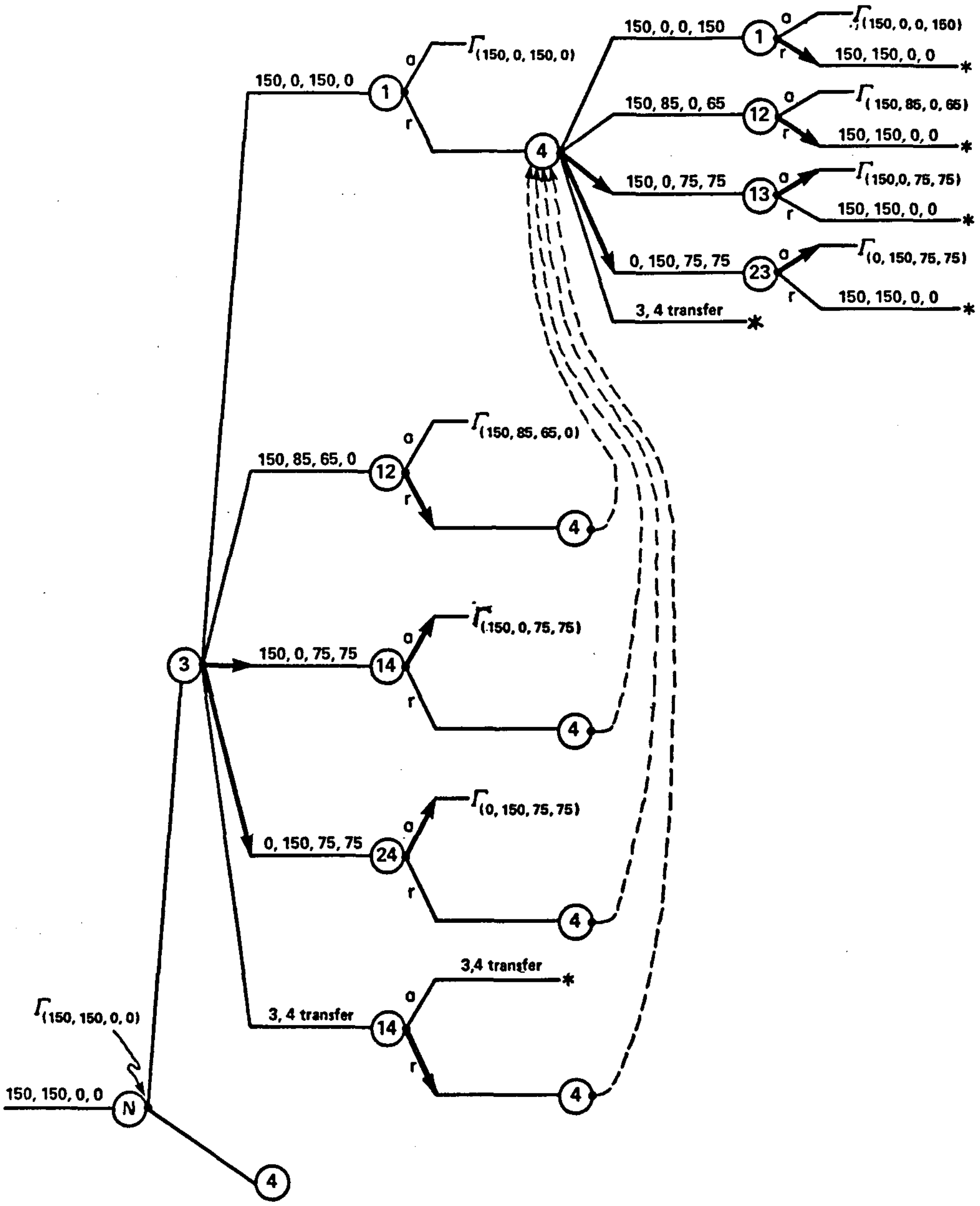
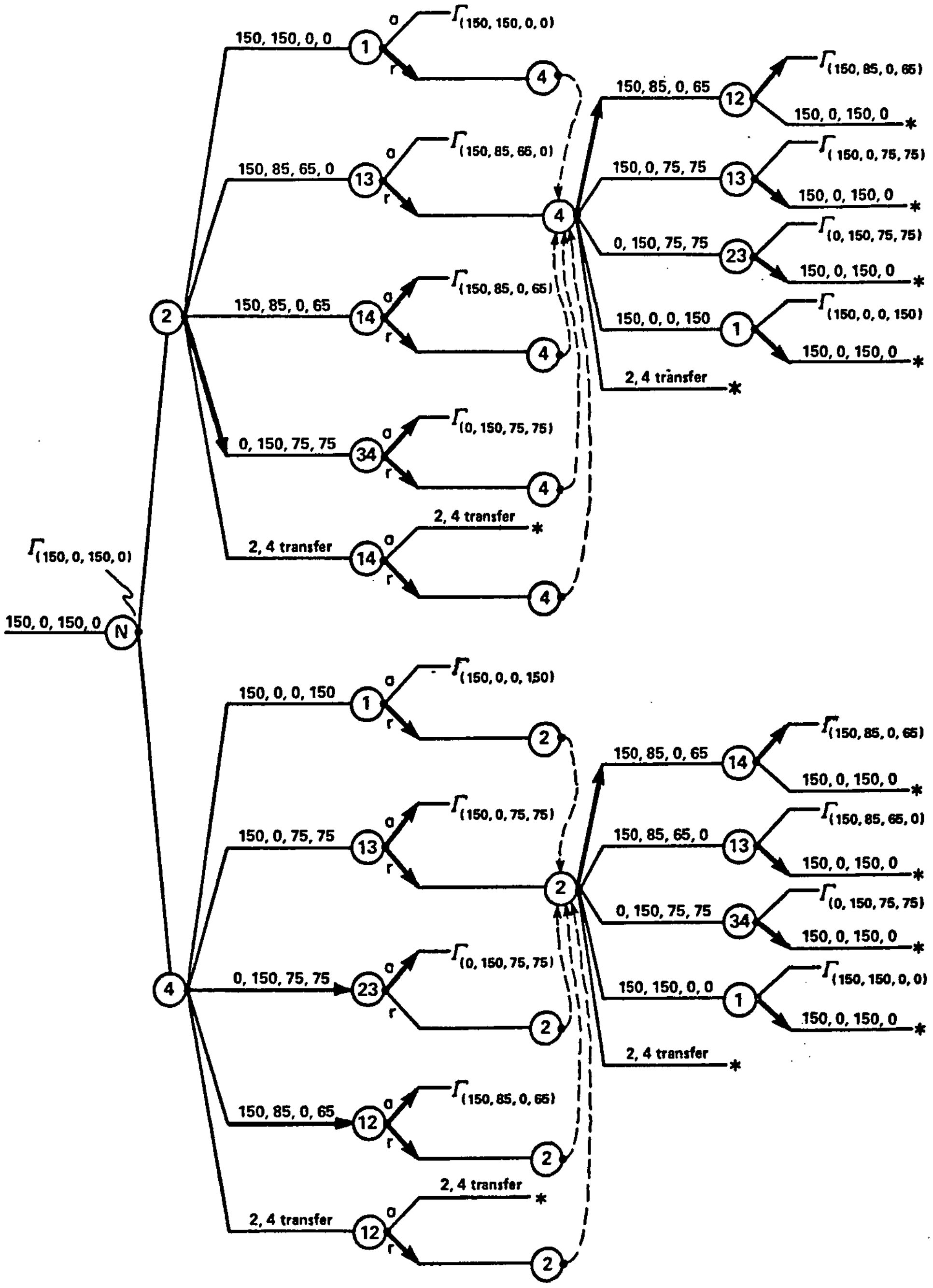

150, 0, 0, 150
1
o
r
Γ(150, 0, 0, 150)
150, 150, 0, 0
*
150, 0, 150, 0
1
o
r
Γ(150, 0, 150, 0)
150, 85, 0, 65
12
o
r
Γ(150, 85, 0, 65)
150, 150, 0, 0
*
4
150, 0, 75, 75
13
o
r
Γ(150, 0, 75, 75)
150, 150, 0, 0
*
0, 150, 75, 75
23
o
r
Γ(0, 150, 75, 75)
150, 150, 0, 0
*
3, 4 transfer
*
150, 85, 65, 0
12
o
r
Γ(150, 85, 65, 0)
4
150, 0, 75, 75
3
14
o
r
Γ(150, 0, 75, 75)
4
0, 150, 75, 75
24
o
r
Γ(0, 150, 75, 75)
4
3, 4 transfer
14
o
r
3,4 transfer
*
4
Γ(150, 150, 0, 0)
150, 150, 0, 0
N
4

Figure 4. Subgame Following the Threat of (150, 0, 150, 0)


Γ(150, 150, 0, 0)
a
150, 150, 0, 0
1
r
4
150, 85, 0, 65
12
Γ(150, 85, 0, 65)
150, 0, 150, 0
*
Γ(150, 85, 65, 0)
a
150, 85, 65, 0
13
r
4
150, 0, 75, 75
13
Γ(150, 0, 75, 75)
150, 0, 150, 0
*
0, 150, 75, 75
23
Γ(0, 150, 75, 75)
150, 0, 150, 0
*
Γ(150, 85, 0, 65)
a
150, 85, 0, 65
14
r
2
4
150, 0, 0, 150
1
Γ(150, 0, 0, 150)
150, 0, 150, 0
*
2, 4 transfer
*
Γ(0, 150, 75, 75)
a
0, 150, 75, 75
34
r
4
2, 4 transfer
*
Γ(150, 0, 150, 0)
2, 4 transfer
14
a
r
4
150, 0, 150, 0
N
Γ(150, 0, 0, 150)
a
150, 0, 0, 150
1
r
2
150, 85, 0, 65
14
Γ(150, 85, 0, 65)
150, 0, 150, 0
*
Γ(150, 0, 75, 75)
a
150, 0, 75, 75
13
r
2
150, 85, 65, 0
13
Γ(150, 85, 65, 0)
150, 0, 150, 0
*
0, 150, 75, 75
34
Γ(0, 150, 75, 75)
150, 0, 150, 0
*
Γ(0, 150, 75, 75)
a
0, 150, 75, 75
4
23
r
2
150, 150, 0, 0
1
Γ(150, 150, 0, 0)
150, 0, 150, 0
*
2, 4 transfer
*
Γ(150, 85, 0, 65)
a
150, 85, 0, 65
12
r
2
2, 4 transfer
*
2, 4 transfer
12
a
r
2

counter that includes $S - C$), 4 is indifferent between offering (0, 150, 75, 75) and (150, 85, 0, 65). But, as before, $v_1(\Gamma_{(150,0,150,0)}) < 150$ and $v_3(\Gamma_{(150,0,150,0)}) < 60$. A parallel analysis holds for (150, 0, 0, 150).

Having established that the posited continuation values are consistent with subgame perfection, we identify a symmetric stationary equilibrium for the full game (by symmetric we mean that all countries abide by the same type of strategy). Consider the following two partial characterizations of equilibria with respect to the game's first move:

Case 1. No player makes or accepts an initial threat unless such action promises a gain.

Case 2. Players make or accept initial threats if doing so promises no loss.

For case 1, countries 1 and 2 prefer coalescing with 3 and 4; but 3 and 4 do not accept any initial offer, because there does not exist an offer that yields them a resource gain. Moreover, neither 3 nor 4 gains by a unilateral defection from its presumed equilibrium strategy. Hence, a situation in which no threats are made is an equilibrium; however, it cannot be a perfect equilibrium.[9] If there is a chance of 4's accepting a threat in which it does not lose, 3 should participate in threats that freeze the system. This argument bears on case 2. If all countries accept threats in which they do not lose, no country has an incentive to switch to a strategy of accepting or making threats only if it gains. Thus, given the limitations on threats we impose, our four-country example, assuming stationary strategies, is system-stable; but only resource instability corresponds to a perfect equilibrium.

There are other threats we must consider before uttering definitive conclusions. Moreover, larger systems allow new possibilities. For instance, if $\mathbf{r}^\circ$ = (110, 80, 60, 30, 20), all countries are essential, but 4 and 5 cannot individually freeze the system. How, then, might 5 respond to a threat of (115, 85, 65, 35, 0)? Or if $\mathbf{r}^\circ$ = (70, 65, 60, 55, 50), no country can buy stability, so might not countries 3, 4, and 5 look favorably on a threat by 1 against 2, since implementation of the threat allows a subsequent defense by, if necessary, a resource transfer? And can 3 respond effectively to a threat by, say, 4 and 5? We cannot answer such questions, using the methods we have applied to systems with three and four countries. We do not have sheets of paper large enough to portray the corresponding extensive forms.

A General Theorem about System Stability

To formulate a general analysis, we partition S into two subsets, L and L_o, where each country in L, but no country in L_o, can be the largest member of a minimal winning coalition. So L_o is losing and consists of the smallest countries, but L_o plus any $k \in L$ is winning (e.g., if the current distribution, $\mathbf{r}^\circ$, equals (120, 100, 80), $L = \{1, 2\}$ and $L_o = \{3\}$). Except for uniform resource distributions (which we preclude with the assumption that $r_i > r_j$ whenever $i < j$), neither L nor L_o is empty; and the significance of these sets stems from the fact that only the members of L will be recipients of a transfer to freeze a system, so that only they can become near-predominant through a transfer. (Countries ought to buy as cheaply as possible, and it is never optimal to freeze the system with a transfer to a country other than the largest member of the threatening coalition.)

With this partition of S, our analysis proceeds thus: We define two classes of continuation values, C1 and C2; and, after isolating a particular set of threats, T^p, from T, we assign one type of value to members of T^p and the other to $T - T^p$. These values and the definition of T^p give T^p *corelike* stability in the sense that no threat in T^p is an effective counter to any other threat in T^p and threats not in T^p are ineffective counters to threats in T^p. Consistency is secured by defining T^p so that two or more of the threats in it are subgame-perfect counters to threats outside of it; thus, the continuation value of a threat not in T^p is a lottery over the values of the threats in T^p that are effective counters to it. For example, in three-country systems {(150, 0, 150), (0, 150, 150)}, combined with the appropriate coalition structures, illustrates T^p. The continuation value we assign to (150, 150, 0) determines that (150, 0, 150) and (0, 150, 150) are effective counters to it, thereby rendering that value consistent with our other assumptions and subgame perfection. We begin, then, with the two types of continuation values, keeping in mind that max[C] is, as before, defined in terms of the status quo resource distribution, $\mathbf{r}^\circ$: ($\mathbf{r}$, C) satisfies continuation condition C1 if

$$v_i(\Gamma_r)\begin{cases} \le r^\circ_i \text{ if } i \in C \cap L_o \\ < R/2 \text{ if } i \in C \cap L \\ \le r^\circ_i \text{ if } i \in C \cap L \text{ and if} \\ \qquad r^\circ_i < r^\circ_{\max[S-C]}; \end{cases}$$

and ($\mathbf{r}$, C) satisfies continuation condition C2 if

$$v_i(\Gamma_r)\begin{cases} = r^\circ_i \text{ if } i \in C \text{ and } i \neq \\ \qquad \max[C] \\ = R/2 \text{ if } i = max[C] \\ \le r^\circ_i \text{ if } i \in S - C. \end{cases}$$

So if a threat ($\mathbf{r}$, C) satisfying C1 is accepted, no country is assured of becoming near-predominant, the members of L_o do not gain resources, and the members of $C \cap L$ cannot gain resources if they are smaller than the largest threatened country. On the other hand, if a threat satisfying C2 is accepted, the largest threatening country becomes near-predominant and the remaining members of the threatening coalition retain the resources they currently control.

Next, we divide threats into the two categories in order to apply C1 to one category and C2 to the other. We proceed in stages, first identifying a type of threat we call a type 1 threat, then selecting a particular subset of these threats. Informally, a type 1 threat ($\mathbf{r}$, C) satisfies four conditions: (1) the largest member of C, max[C], can coalesce with $S - C$ to form a new winning coalition; (2) members of $S - C$ are threatened with elimination; (3) the threat proposes to render the largest country in C near-predominant; and (4) there is no other winning coalition C' such that C' and C share only one member where that member is the largest country in C' but not in C.[10] Formally, ($\mathbf{r}$, C) is a type 1 threat—($\mathbf{r}$, C) $\in T^1$—if

1. $r^\circ_{\max[C]} + r^\circ(S - C) \geq R/2$
2. $r_j < r^\circ_j$ for all $j \in S - C$
3. $r_{\max[C]} = R/2$
4. There does not exist $C' \in W$ such that $C' \cap C = \{k\} = \{\max[C']\} \neq \{max[C]\}$.

Condition 3 removes threats like ((75, 150, 75,0), {1, 2, 3}) from the list of type 1 threats since max[C] = 1, but $r_{\max[C]}$ is not equal to 150. Condition 4 plays a role in three- and four-country systems; but in, for example, a five-country system in which $\mathbf{r}^\circ$ = (70, 65, 60, 55, 50), threats such as ($\mathbf{r}$, C) = ((150, 75, 75, 0, 0), {1, 2, 3}) are eliminated because with C' = {2, 4, 5}, the intersection of C and C' is {2} and country 2 is the largest member of C' but not of C.

Next, we identify a subset of type 1 threats that exhibits corelike stability. Briefly, T^p is a *set of primary threats* if it satisfies two conditions. First, for no two threats ($\mathbf{r}$, C) and ($\mathbf{r}'$, C') in T^p is it the case that C and C' have a unique common member who is also the largest member of

C and C'. Thus, in our four-country example, (150, 0, 150, 0) and (150, 0, 0, 150) cannot both be in T^p. This requirement, after continuation values are assigned, ensures that no member of T^p is an effective counter against other members of T^p. Second, we require that T^p be maximal in the sense that there not exist a winning coalition with a type 1 threat that can be formed by adding any member of $S - C$ to C, which, in the proofs of our results, assures us that nothing is precluded from T^p that cannot be effectively countered. Again looking at a four-country example, we cannot have {(150, 0, 150, 0), (150, 0, 0, 150)} as a set of primary threats, since we can add (150, 0, 75, 75). Formally, letting $\mathbf{T}_0$ be the power set of T^1 (the set of all subsets of T^1), $T^p \in \mathbf{T}_0$ is a set of primary threats if

1. For no $(\mathbf{r}, C) \in T^p$ is there an $(\mathbf{r}', C') \in T^p$ such that $C \cap C' = \{\max[C]\} = \{\max[C']\} = k$.
2. For $(\mathbf{r}, C) \in T^p$ there does not exist j in $S - C$ such that $\mathbf{r}_{\max[C \cup \{j\}]} + r(S - C - \{j\}) \geq R/2$.

Notice that this definition renders T^p necessarily unique. However, before illustrating these definitions, we first establish that for any set of primary threats, each i in L is part of at least one threat in which it is the largest member of the threatening coalition and every i in L_0 is part of a threat in which it is included in the threatening coalition. Formally,

LEMMA 1. *For each $i \in L$, there is at least one $(\mathbf{r}, C) \in T^p$ such that $i = \max[C]$ (in particular, $\{i\} \cup L_0$, $i \in L$, has a threat in T^p); and for each $j \in L_0$, there is at least one $(\mathbf{r}, C) \in T^p$ such that $j \in C$.*[11]

To illustrate, {(150, 0, 150), (0, 150, 150)}, with the appropriate coalitions, is the unique set of primary threats in a three-country system. In our four-country example, both {(150, 0, 75, 75), (150, 85, 65, 0), (150, 85, 0, 65), (0, 150, 75, 75)} and {(150, 0, 150, 0), (150, 0, 0, 150), (150, 0, 75, 75), (0, 150, 75, 75)} are sets of type 1 threats; but only the first is a primary set, because (150, 0, 150, 0) and (150, 0, 0, 150) cannot exist in any $T^p \in \mathbf{T}_0$.[12] Finally, let $\mathbf{r}^\circ = (70, 65, 60, 55, 50)$, so from condition 1 in the definition of T^1, threats in T^p must originate with coalitions having three members. Now consider the following distributions, which, with appropriate coalitions, satisfy the definition of a type 1 threat:

1. (150, 0, 75, 75, 0)
2. (150, 0, 75, 0, 75)
3. (150, 0, 0, 75, 75)
4. (0, 150, 75, 75, 0)
5. (0, 150, 75, 0, 75)
6. (0, 150, 0, 75, 75)
7. (0, 0, 150, 75, 75)

There are other threats we might consider, such as (150, 75, 75, 0, 0) by {1, 2, 3}; (150, 75, 0, 75, 0) by {1, 2, 4}; and (150, 75, 0, 0, 75) by {1, 2, 5}. But each violates condition 4 in the definition of T^1; thus, they are not candidates for inclusion in T^p. Indeed, the seven listed threats constitute the unique set satisfying the definition of T^p.

We proceed now to our result about stationary equilibria using two lemmas that establish the subgame-perfect consistency of associating continuation value condition C2 with the set of primary threats and condition C1 with all remaining threats. Our second lemma, then, states that if $(\mathbf{r}, C)$ is a primary threat and all other primary threats satisfy C2 and all nonprimary threats satisfy C1, $(\mathbf{r}, C)$ satisfies C2.

LEMMA 2. *If all threats not in T^p satisfy C1 and if all threats in $T^p - \{(\mathbf{r}, C)\}$ satisfy C2, then for any stationary equilibrium, $(\mathbf{r}, C) \in T^p$ satisfies C2.*[13]

The difficulty here, of course, is that we have not yet established any result validating the assumption that C1 holds for nonprimary threats. In fact, the proof that C2, the consequences of Lemma 2, and subgame perfection are consistent requires an assumption bearing specifically on the nature of the stationary equilibrium that we can sustain. We begin with the observation that if a country is confronted with a threat and it cannot counter so that its loss of sovereignty is inevitable, it is indifferent among the various actions that it takes at that point. Thus, the actions countries choose in such circumstances are free parameters. It might seem, of course, that such free parameters are irrelevant to final conclusions. But it is important to understand that other countries may not be indifferent as to which action a given country chooses when confronting elimination; and what they believe about the eventual choice of the threatened country can determine their prior actions. Indeed, these beliefs can even determine whether the country in question will confront the possibility of elimination.

This discussion leads to the notion of a *credible threat*. Briefly, a threat is credible if (1) it is part of a subgame-perfect equilibrium (that is, at the time of its implementation, no other choice yields a greater payoff to the country in question) and (2) the belief by others that the threat will be chosen improves the utility of the country that might choose it. In regard, then, to the actions of countries threatened with inevitable elimination, the assumption we use to characterize a stationary equilibrium is

CC1. *If* $S - C$ *has no counter to its threatened elimination and if it cannot buy stability directly,* $S - C$ *transfers all of its resources to max*$[C]$.

This assumption is not without historical challenge. Although Nazi leaders at the final stages of World War II preferred capitulation to the United States and Britain rather than to Russia, the Allied powers largely controlled the final allocation of German resources. On the other hand, in addition to the imperatives of the atomic bomb, Japan chose a timely surrender because of its desire to capitulate to the United States rather than contend fully with Russian territorial ambitions.

A second assumption about equilibrium strategies, introduced in our discussion of four-country systems, facilitates our proofs. Let $(\mathbf{r}, C)$ be the current threat and let $i \in S - C$ offer a counterthreat. Any counter, of course, must either entail a transfer of resources or (since C is necessarily winning) coopt one or more members of C into a new coalition, C'. Our assumption is that if $i \in S - C$ can form a counter that coopts only one member of C so that all of i's other coalition partners in the counter are in $S - C$, then, ceteris paribus, i chooses that counter. This is not to say that i ignores other alternatives if they are more valuable but that whenever it is indifferent, i takes advantage of the fact that $S - C$ is a coalition that because of C's threat, is "already nearly formed." Hence,

CC2. *If* $(\mathbf{r}, C)$ *is the current threat, then, ceteris paribus,* $i \in S - C$ *chooses a counter,* $(\mathbf{r}', C')$, *such that* $C' \cap C = \{j\}$, *and* $C' - \{j\} \subseteq S - C$.

We can now complete the specification of consistent continuation values with a lemma stating that if conditions CC1 and CC2 apply and all primary threats satisfy C2 and all nonprimary threats—except possibly $(\mathbf{r}, C)$—satisfy C1, $(\mathbf{r}, C)$ must satisfy C1:

LEMMA 3. *If* T *in* T^p *satisfies C2, if* $(\mathbf{r}, C) \notin T^p$ *and if all other threats satisfy C1, CC1 and CC2 characterize equilibrium strategies for* Γ *such that* $(\mathbf{r}, C)$ *satisfies C1.*[14]

Lemma 3 exhausts all possibilities not encompassed by Lemma 2; thus, together they establish consistent continuation values for all subgames of Γ.[15]

We turn now to our central result—the characterization of a stationary equilibrium. What remains at issue is a specification of a country's choice whenever it is selected to make the initial threat and the responses of its partners in a proposed initial threat. Postponing the question of the fate of inessential countries, assume that all S are essential. Limiting the discussion to symmetric strategies (strategies in which all countries in L and all those in L_0 abide by the same strategy), consider this statement as a characterization of equilibrium:

> CC3. *If $i \in S$ is chosen to make the initial threat, i randomly chooses* $(\mathbf{r}, C) \in T^p$, *$i \in C$, and all $j \in C - \{i\}$ accept; and in particular, if $i \in L$, i chooses* $(\mathbf{r}, C)$ *such that $i = max[C]$.*

So if $i \in L$ is chosen by nature to make the initial threat, i proposes a primary threat in which it is the largest member of the threatening coalition; if $i \in L_0$ is chosen, i proposes a randomly selected primary threat in which it is a member of the threatening coalition and all members of the proposed threatening coalition accept.

To see whether this characterization describes an equilibrium, notice that Lemmas 2 and 3 establish that if all other countries abide by CC3 and if $i \in L$ can make the initial threat, i's dominant choice is to select a threat $(\mathbf{r}, C) \in T^p$ such that $i = \max[C]$. This choice renders i near-predominant, and achieving near-predominance is the best feasible outcome for any country. Both lemmas also imply that no country in $C \cap L_0$ has an incentive to defect from accepting. (They can never gain thereby, and acceptance ensures that they cannot lose.) Finally, suppose one of i's partners in C, say j, is in L. The question is whether j, rather than abiding by CC3, has an incentive to defect unilaterally to a strategy whereby it rejects i's offer. Keeping in mind that j neither gains nor loses resources if it accepts, the benefit of defecting is that j might be selected subsequently to make an initial threat or else some country in L_0 might be selected to make a new initial threat, and this threat might be one in which j is the largest member of the threatening coalition. On the other hand, the hazards of defecting are that j will otherwise be the target of any initial threat. Complex expected resource calculations, however, are unnecessary. First, since we assume that all aspects of the game, including people's attitudes toward risk, are common knowledge, i knows beforehand whether or not j will accept its offer to participate in a threat. So if j would accept i's offer in accordance with CC3, either because j is sufficiently risk-averse or because the probabilities of the various outcomes imply that j's gain from defection is negative,[16] the following result holds:

> STABILITY THEOREM. *If all $i \in S$ are essential, CC1–CC3, in conjunction with C1 and C2, describe a stationary equilibrium for Γ such that the system* $(S, \mathbf{r}°)$ *is system-stable; but if $r°_i < R/2$ for all $i \in S$,* $(S, \mathbf{r}°)$ *is resource-unstable.*

With respect to stationary equilibria characterized by the statement "No country makes or accepts threats unless doing so promises an immediate gain," let $\mathbf{r}° = (70, 65, 60, 55, 50)$. Since no threat in T^p is accepted, let 1 threaten (120, 65, 60, 55, 0). Ignoring the incentives that 2, 3, and 4 might have for avoiding a four-country game, 5 is eliminated, because 2, 3, and 4 are unwilling to join in a counter in T^p unless such action promises a gain to each of them and because all threats here have three members in C. On the other hand, 5 cannot individually threaten anyone;

whereas if it can make the initial threat and there is some chance that others will accept a threat in T^p, 5's dominant strategy is to make a threat in T^p in which $\max[C] = 2$. Clearly, 2 has an incentive to accept, and 5's other partners should accept as well, since doing so ensures that they cannot lose resources. (Rejection would merely allow a larger country in the next round to threaten them with elimination.) Hence, the presumed equilibrium cannot be perfect.

Collective Security

Having established that balance of power as traditionally envisioned yields system stability if all countries are essential, we turn now to the notion of collective security (modeled by punishment strategies) and to the question whether such strategies, too, can sustain stability. First, we must specify more precisely what we mean by a punishment strategy. Letting D denote the set of players who have previously defected from the strategy specified, we restrict our attention to the following characterization:

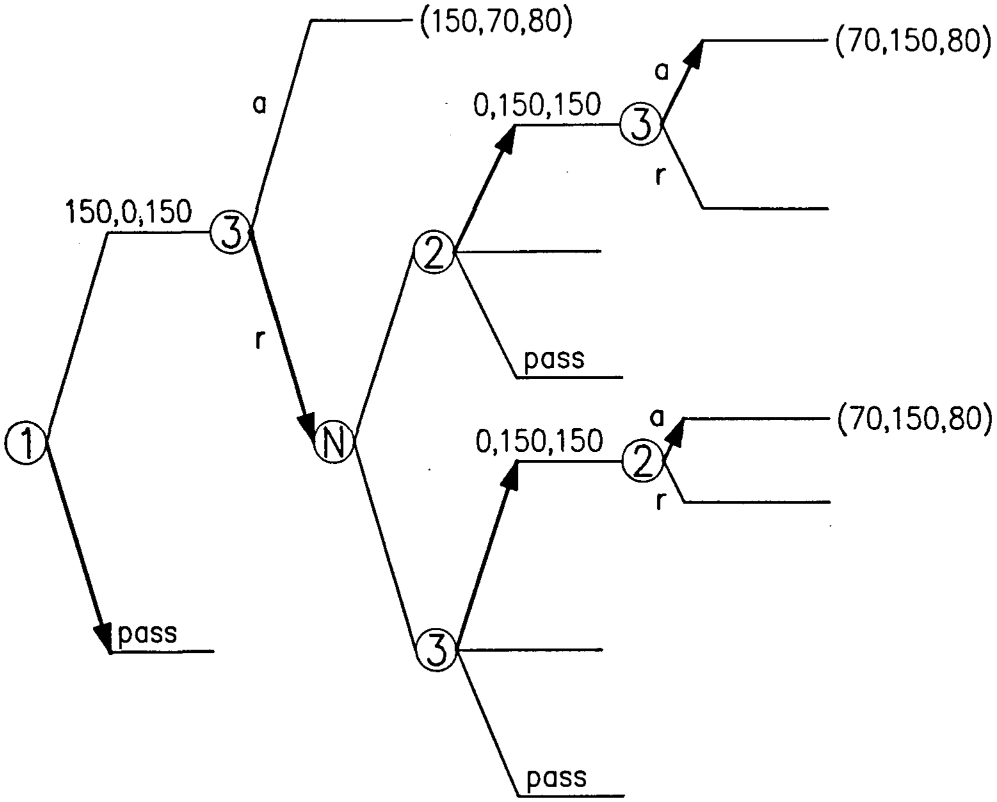
1. No country proposes an initial threat.
2. No country accepts an initial threat if one is offered.
3. Threats are directed against one or more members of D.
4. Countries accept threats that are punishments.
5. Whenever any threat is accepted, all countries use stationary strategies thereafter.

Players defecting from characterization 1–5 are added to D and are thereafter subject to punishment.

To simplify matters, we suppose now that after any threat is rejected, nature chooses from among the set of players who have not previously defected. Now, to illustrate matters with a three-country system, let $r^\circ = (120, 100, 80)$ and consider country 1's incentives to defect from characterization 1 by proposing (150, 0, 150). Referring to Figure 5, if 1 defects—and if 3 defects also by accepting—then, in accordance with characterization 5, play reverts to stationary strategies and the continuation value (150, 70, 80) applies. Suppose, then, that 3 rejects and that nature chooses 2 to make the next move. At this point we need not concern ourselves with what follows if 2 defects by passing or by proposing, say, (150, 150, 0). We know that 2 cannot get more than 150; nor can 3 get more than 80, since it is never the recipient of a transfer. Hence, if 2 proposes to punish 1 with (0, 150, 150), 3 has no incentive to defect by rejecting this punishment—which, of course, makes the punishment the optimal choice for country 2. If we work backward up the tree in Figure 5 (arrows indicate subgame-optimal choices, which are also consistent with the presumed equilibrium), we discover that although 3 is indifferent at various points, it has no incentive to defect from the presumed collective security equilibrium and that, in particular, it has no positive incentive to accept 1's initial offer; so 1 does not defect.

If, on the other hand, we allow country 3 to have the first move, we can begin to detect problems with a collective security equilibrium. Referring to Figure 6, where dashed lines denote the presumed equilibrium path choices, suppose that whenever a country is indifferent between two choices off the equilibrium path, it chooses between them with equal probability. Also, suppose that whenever 3 can freeze the system and assure that it will not be punished, it does so. Now let 3 threaten (0, 150, 150). If 2 also defects by accepting 3's offer, characterization 5 implies that (70, 150, 80) prevails. So suppose that 2 rejects and that 1, acting in accordance with characterization 2 to punish 3, threatens 3 with (150, 150, 0). If 2 defects by rejecting, 1 moves again

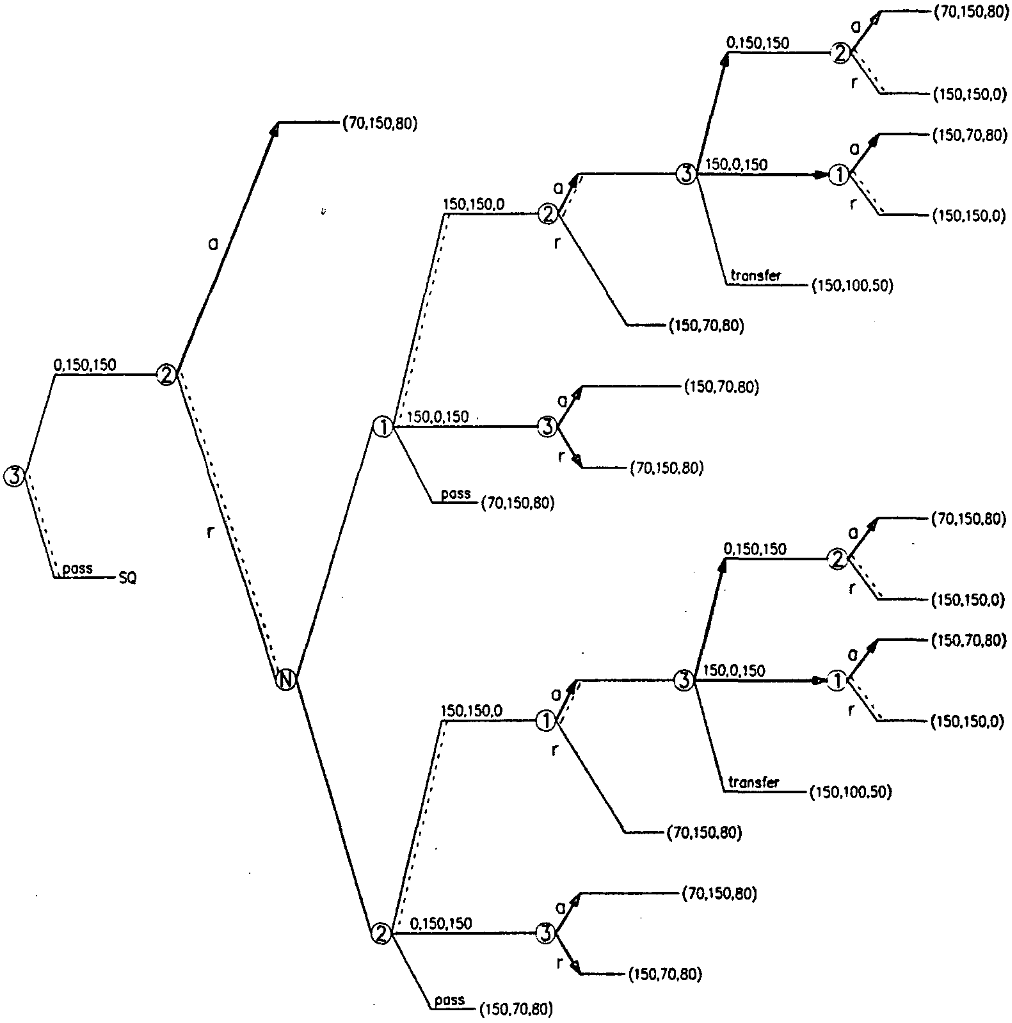
Figure 5. Punishment Responses Following the Threat of (150, 0, 150)



(since it is the sole nondefecting player) and (150, 70, 80) prevails—because everyone plays stationary strategies at this point and, from our previous analysis, 1 threatens (150, 0, 150), 3 accepts, and 2 transfers to 1. But if 2 accepts, 3 must counter with (150, 0, 150) or (0, 150, 150) or transfer to 1. If 3 counters with (150, 0, 150), 1 accepts because, from characterization 5, (150, 70, 80) prevails. Similarly, if 3 proposes (0, 150, 150), 2 accepts. Since 3 is indifferent between these two threats, it chooses between them with equal probability, thus giving 2 the expected return of 110, which 2 prefers to 70. Thus, 1's attempt at punishing 3 yields a lottery between (70, 150, 80) and (150, 70, 80). On the other hand, a defection by 1 to (150, 0, 150) yields a lottery between (70, 150, 80) and (150, 70, 80); so 1 has no positive incentive to defect and chooses (150, 150, 0). Similarly, if nature chooses 2, it proposes (150, 150, 0). This means, however, that 2's initial response to 3's proposal is to accept that proposal, since it gives 2 a payoff of 150 with certainty. So punishment is not subgame-perfect.

This analysis does not wholly undermine the possibility of a collective security equilibria: if 3 assumes that 1 and 2 will abide by their punishment strategies, country 3, like 1 and 2, will not defect with an initial proposal. And even if 3 believes that 1 or 2 will accept a proposal, it has no positive incentive to offer one. We can, in fact, see reasons for believing

Figure 6. Punishment Responses Following the Threat of (0, 150. 150)


(70,150,80)
0,150,150
a
2
r
(150,150,0)
(70,150,80)
(150,70,80)
a
3
150,0,150
1
r
(150,150,0)
150,150,0
a
2
r
transfer
(150,100,50)
(150,70,80)
0,150,150
2
a
r
(150,70,80)
1
150,0,150
3
a
r
(70,150,80)
pass
(70,150,80)
3
pass
SQ
N
(70,150,80)
0,150,150
a
2
r
(150,150,0)
(150,70,80)
3
150,0,150
1
a
r
(150,150,0)
150,150,0
a
1
r
transfer
(150,100,50)
(70,150,80)
(70,150,80)
2
0,150,150
a
3
r
(150,70,80)
pass
(150,70,80)

that the simple punishment strategies we consider can result in resource stability and system stability for larger games. The particular feature of a three-country situation that renders the idea of collective security problematic is that if country 3 defects by proposing an initial threat, its partner in the threat can ensure a resource transfer to itself by accepting. However, it is generally the case in larger games that two or more countries must defect to render a primary threat the current threat; and unless they somehow coordinate to accept simultaneously, unilateral acceptance places a country in jeopardy. Elsewhere, we generalize this conclusion (Niou and Ordeshook 1991) by establishing the general existence of a collective security equilibrium in the context of our model of anarchy. Thus, collective security "works," but only if there are more than three countries in the system.

Despite such an argument, we can also see from our example why collective security equilibria are more vulnerable than the equilibria supported by stationary (balance-of-power) strategies. Unlike a balance-of-power equilibrium with stationary strategies, a collective security equilibrium is not only not subgame-perfect, it is not *strong*. Indeed, in our three-country example, if 3 has any preference for a system that is frozen, our punishment strategy fails to yield resource stability. Further, recall that 1 is deterred from making an initial threat because 3 has no *strictly* positive incentive to defect from characterization 2; but if we allow even an infinitesimal side payment to 3 from 1 to accept 1's offer, the collective security equilibrium is destroyed. A similar infinitesimal side payment to 3 if it makes the initial threat also destroys the equilibrium. Naturally, such side payments change the formal character of our game; but equilibria supported by stationary strategies do not rely on such specific choices at points of indifference and so are less sensitive to "knife-edged" conditions.

The Possibility of Instability

Because our stability result supposes that all countries are essential, we cannot preclude the survival of inessential countries. Of course, our analysis of four-country systems illustrates the elimination of such a country; but four-country systems are special and our model must be modified before we can eliminate inessential countries in general. To see why such systems are special, suppose that $\mathbf{r}^\circ = (100, 90, 80, 30)$ and that 3, making the initial threat, proposes (110, 100, 90, 0). If 1 and 2 accept, 4 has no useful counter: any counter in T^p must include 3; but 3 rejects such counters, since it cannot gain resources with them and since 3 cannot lose resources in a three-country system if (110, 100, 90, 0) prevails. Alternatively, if 4 proposes a counter with 1 and/or 2 against 3, this leaves 1 and 2 vulnerable to a counter in T^p by 3 that requires one or the other to transfer resources; and since the game between them is zero-sum, one or the other will refuse to accept 4's proposal. With these consequences in mind, 1 and 2 accept 3's initial offer, since even if one or the other must transfer in the three-country game that follows, each must transfer less with 4 eliminated than otherwise.

This reasoning, though, cannot be extended to a five-country system in which one country is inessential, such as $\mathbf{r}^\circ = (100, 70, 60, 55, 15)$. If 4 initially threatens, say, (105, 75, 63, 57, 0), then, in accordance with CC1, 5 transfers to 1 and is eliminated. But now 4 confronts the possibility of having to transfer resources in the four-country game that ensues. Thus, since 4 cannot gain by threatening 5 alone and can lose resources if it fails to freeze the system, it (as well as 1, 2, and 3) strictly prefers an initial threat in T^p. What makes a four-country system with an inessential country different from larger systems, then, is that the unique essential country in L_0 cannot lose resources in the three-country game that follows the elimination of the inessential player, whereas all countries can lose resources in larger systems. Thus, there is an irresistible incentive for countries in large systems, when initially selected by nature, to choose primary threats that freeze the system. Countries in L will not forego the opportunity to become near-predominant, whereas countries in L_0 can be certain not to lose resources only if someone is rendered near-predominant. As a consequence of this "rush to stability," essential countries in L and L_0 sacrifice the possibility of wholly absorbing inessential countries.

To see, however, how a modification of our analysis leads to the elimination of small countries, suppose that 4, in our five-country example, can propose a "sequential" initial threat, namely, "3 and

4 eliminate 5 and distribute its resources between themselves, then require that 2 transfer so as to render 1 near-predominant; but if 2 rejects the transfer, implement (150, 0, 75, 75, 0)." If this threat "works" (in the sense that 1 is rendered near-predominant) 1 ought to accept 4's offer. And although 3 might prefer a different share of 5's resources than the one 4 proposes, it should accept participation in the threat for the reasons we specified in the discussion of our Stability Theorem.[17] At issue, then, are 2's and 5's responses to 4's sequential threat. Because we want to show that inessential countries *can* be eliminated, it is sufficient to find a circumstance under which elimination occurs. So, supposing that 2 is first to counter 4's threat, 2 must transfer, since it has no viable counter in T^p. But if 2 tries to save resources by proposing a transfer in which some of 5's resources are ceded to 1, 1 is indifferent between accepting or rejecting; whereas 5 can reject, secure the last move in the counterthreat sequence, and propose that 2 alone render 1 near-predominant (at which point 2 accepts, since rejecting implements (150, 0, 75, 75, 0)). Thus, 2 has nothing to gain by not acceding to the transfer, and we can assume that it accedes in equilibrium.

If we represent 2's choice by the decision to pass, 5 must counter next. Like 2, 5 has no counter in T^p, so it must consider transfers. A proposal by 5 to cede resources with 2 to 1 offers 1 no improvement over what it gets from 2 alone; thus, we can assume that in equilibrium, 1 rejects 5 (and a preemptive transfer by 5 to 1 meets the same fate). Since countries are risk-averse, 5 cannot try to disrupt matters with a preemptive transfer of something less than r_5 to 3 or 4. Thus, 5 is eliminated.

One final consideration remains—whether the revision of the form of initial threats yields the elimination of essential countries. Let $\mathbf{r}^\circ$ = (70, 65, 60, 55, 50) and let 4 propose, as before, to eliminate 5 and threaten 2 so as to secure an outcome such as (150, 10, 75, 65, 0). Suppose 2 counters first in the same way—acceding to the threat. But now 5's resources are essential to 2's ability to freeze the system; and 5, after 2, can propose a transfer to 1 in which 2 shares most of the burden. Clearly, 1 accepts 5's offer, since it does not require implementation of a threat; and the final outcome (namely, 2 and 5 transfer to 1) is the same as when we allowed only simpler initial threats.

However, not all essential countries survive sequential threats. Let $\mathbf{r}^\circ$ = (150, 122, 24, 8, 6) and let {1, 3, 4} threaten {2, 5}. Country 5's problem, now, is that 5's resources are inessential to 2's ability to render 1 near-predominant; thus, as with inessential countries, a sequential threat eliminates 5. It is important to keep in mind, however, that the game Γ is not intended to model the entire process of international politics. Hence, we can imagine a "pregame" in which countries 4 and 5, anticipating their mutual vulnerability in Γ (just as the threat by {1, 3, 4} eliminates 5, a threat by {1, 3, 5} eliminates 4), coalesce to form a confederation or a new country. The incentives to form such a confederation, of course, lie in the fact that if $\mathbf{r}^\circ$ = (140, 122, 24, 14), no country is subsequently eliminated, because all countries can individually buy stability. Thus, reminiscent of Riker's (1964) analysis of federalism, we detect in our analysis forces promoting the formation of new states. Notice, moreover, that confederations cannot ensure the sovereignty of inessential countries, because a confederation of such countries is itself inessential. Thus, we can reassert the conclusion of our previous analyses that inessential countries are eliminated. Of course, this discussion is no substitute for a formal analysis; but we now see how our analysis can be modified to yield insights into processes that otherwise seem outside the realm of realist theory.

Conclusions

In drawing lessons about war and peace from our analysis, we must keep in mind that since we do not model the substantive form of a threat's implementation, we cannot be certain that the stability we deem possible in anarchic systems implies the absence of wars. Collective security equilibria, of course, imply the absence of threats; but we have indicated why we believe such equilibria are more fragile than those supported by stationary strategies. Substantive conclusions must be treated tenuously, moreover, because our analysis fails to accommodate important features of international politics, including uncertainty, the strategic complexity that uncertainty allows, disjunctures between military and economic capabilities, ambiguities in the notion of sovereignty, and the possibility of people's pursuing goals that do not translate readily into some notion of power maximization. Our analysis also implies that barring nonterminal transfer cycles, systems are quickly frozen, with one country becoming near-predominant—which underscores the fact that our model does not yet accommodate the unequal resource growth of countries, which can render systems unstable (but see Niou and Ordeshook 1987 and Niou, Ordeshook, and Rose 1989).

Our stability theorem is nevertheless important. Whether labeled *tzung-hun* in the Warring States Period of China, identified as the policy of a Great Power in the nineteenth century, or renamed realism in this century, there is the continuing attraction and salience of the idea that some notion of balance of power is an essential source of stability. However, there has long existed one problem for adherents to this view—the absence of any formal, deductive proof that there exists an interpretation of balance of power that would ensure stability. Our conceptualization of anarchy and our Stability Theorem offers such a proof. One can argue, of course, that our theorem is merely a possibility result and that its descriptive relevance is questionable; but with that theorem we can address secondary issues that arise in the context of realist thinking. For example, we see that stability does not require any specific number of countries or great powers, nor does stability require either a uniform or a highly asymmetric resource distribution. We have also seen that the allowable form of initial threats can alter our assertions about stability. Thus, our conclusions are sensitive to a variable that had not previously been appreciated. Finally, looking at the threats and counters in T^p, which are the primary instruments of resource reallocations, the target countries must be large enough that they can render max[C] near-predominant. Thus, although we do not predict initial threats that correspond identically to Riker's (1962) size-principle hypothesis, we anticipate alliances that correspond approximately to this hypothesis.

Despite our emphasis on anarchic systems, our model suggests a profound role for institutions as agents for facilitating stability. Although collective security equilibria are weak as enforced by the particular punishment strategy we consider, the existence of multiple equilibria raises the possible necessity for coordination in order to ensure that some equilibrium prevails. Institutions, of course, can greatly facilitate this coordination effort. In addition, institutions play a role with respect to the issue of common knowledge. Common knowledge is an essential assumption in our analysis: our conclusions follow only if all decision makers know the game's structure (the resources of other countries and the consequences of alternative choices), know that all others know, know that all others know that all others know, and so on. However, despite Blainey's (1973) arguments about the consequences of violations of this assumption (a necessary and perhaps sufficient condition for war), we know little about the mechanisms that facilitate common knowledge. In vague terms, however, we know that strategic interaction and communication are "useful." We may not understand in a rigorous sense how institutions facilitate common knowledge, but they are almost certainly essential toward that end. Minimally, our analysis identifies the variables and concepts that ought to be common knowledge if the stabilizing processes of balance of power are to operate; thus, we can identify the purposes that some institutions and processes might be designed to serve.

Our model also highlights the role of attitudes toward risk and the corresponding relevance of domestic politics. We assume throughout that decision makers are risk-averse with respect to elimination. Nothing we have said about system stability applies, however, if people are risk-acceptant; indeed, risk acceptance almost certainly destabilizes systems. Thus, to the extent that attitudes toward risk are determined by domestic politics (to the extent, for example, that decision makers equate their personal survival with their country's sovereignty so that the pursuit of dangerous foreign adventures is seen as essential to maintaining domestic power), otherwise stable systems are destabilized. We sympathize, then, with the analyses that interpret Hitler's personality and the political economy of Weimar Germany as critical to the outbreak of World War II rather than some breakdown in traditional balance-of-power forces (see Mueller 1989).

Many questions remain. Are there alternative classifications of threats and continuation values that yield different conclusions? What outcomes can we sustain with more complicated punishment strategies? How sensitive are our conclusions to change in Γ's extensive form? Such questions, of course, emphasize that the preceding analysis is but a tentative first step toward a theory of international processes. But we are also reminded that the realist, neorealist, liberal, and neoliberal literatures are replete with inconclusive arguments about the factors that facilitate or inhibit stability. And although a great many extensions of our analysis are required before definitive conclusions can be reached, the complexity of that analysis is evidence that verbal argument is unlikely in principal to be definitive. At least the formal analysis offered here makes us certain of one fact, namely, that there exists one world—albeit reminiscent of the frictionless planes that introduce physics—in which the sovereignty of essential participants is assured in anarchy.

Notes

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1. The results to date are incomplete; but we now know, for example, how cooperation is sustained in the repeated prisoner's dilemma (Axelrod 1984; Taylor 1976), and how repetition and uncertainty sustain vote trading in legislatures (Calvert 1989). And we know, in some circumstances, how to rationalize cooperative solution hypotheses as noncooperative equilibria (Selten 1981; Sutton 1986).

2. In addition to assuming that $r°$ strictly orders S, we assume that there are no blocking coalitions except when $r°_i = R/2$. Neither assumption is of any consequence to our results, but their imposition greatly simplifies argument. However, we should comment on our assumption that the total resources in the system, R, is constant as it pertains to the issue of national goals. Specifically, even if we make suitable allowances for the goal of ensuring one's sovereignty, there remains considerable discussion in the realist-neoliberal debate about whether it is best to conceptualize countries as power-maximizers or as absolute wealth-maximizers (see Grieco 1988; Keohane 1989). We do not want to enter this debate here; but notice that with R constant, both goals are equivalent. For the argument that these goals ought

to be endogenously determined and for a discussion of the implications of our analysis to that debate see Niou and Ordeshook 1991.

3. Assuming $j \in C$ if $r_j = r^\circ_j$ precludes some silly possibilities, such as the possibility that after {1, 2, 3} threatens {4, 5}, {4, 5} counters by threatening {3}, or {4} counters by threatening {5}.

4. See, for example, Luce and Raiffa 1957, 461–76; Owen 1982, 97–100; and Shubik 1982, 53–54. For their political application, see Baron and Ferejohn 1989. We note that readers may be more familiar with an analysis of repeated games in which the stream of payoffs from repeated play are discounted into the future so as to render present values finite. In our game, however, there generally is no stream since resources change only when counterthreats in the form of resources transfers or threats themselves are implemented. Indeed, all action may simply take place in the heads of participants as they contemplate possibilities, in which case there is no natural structure to the discounting that can be applied.

5. Our analysis, although similar to Wagner's, differs from his in several ways. First, Wagner offers no general results about n-country systems, proceeding instead on a case-by-case basis. However, we do not accomplish generality costlessly. Wagner pays greater attention to the mechanisms whereby nations war and secure resources, whereas we assume simply that uncountered threats are implemented. Another difference is that we allow preemptive resource transfers. Wagner argues against this assumption, but such transfers seem an important part of international processes. As Morgenthau (1959, 167–68) observes, "Compensations of a territorial nature were a common device in the eighteenth and nineteenth centuries for maintaining a balance of power . . . [and] the bargaining of diplomatic negotiations, issuing in political compromise, is but the principle of compensations in its most general form, and as such it is organically connected with the balance of power."

6. In order to make this risk aversion assumption more palatable, notice that if such a proposal is offered and accepted, it merely results in a new four-country system and that if all such offers are accepted, we can cycle endlessly without eliminating countries, in which case $(S, \mathbf{r}^\circ)$ is system- but not resource-stable. However, because we are especially interested in system stability and because elimination, if it occurs at all, occurs only if countries are unwilling to propose or accept such nonterminal transfers, we suppose that such transfers will not be made or accepted and analyze the part of the extensive form in which transfers freeze systems or countries are eliminated.

7. We can extend the previous analysis to show that (150, 0, 150, 0) yields $(150, 75 - \epsilon, 75, \epsilon)$ or $(150, 45 + \epsilon, 75, 30 - \epsilon)$; whereas (0, 150, 150, 0) yields $(75 - \epsilon, 150, 75, \epsilon)$ or $(45 + \epsilon, 150, 75, 30 - \epsilon)$. For example, if 3 counters with (150, 0, 150, 0), the final outcome depends on whether 2 or 4 has the last move in offering a counter, which necessitates a transfer. If 2 moves last, it can propose $(150, 75 - \epsilon, 75, \epsilon)$, and 4 has little alternative but to accept; whereas if 4 moves last, it can propose $(150, 45 + \epsilon, 75, 30 - \epsilon)$.

8. If 4 is threatened with elimination and cannot find a counter, it is indifferent among all moves, including a preemptive transfer to, say, 1. Later, we characterize equilibria with such an assumption; but here this transfer is of no consequence. It eliminates 4; and since 3 makes or accepts threats if doing so yields no loss, 3 proceeds with the threat. In the resulting three-country system (130, 95, 75), 3 cannot lose resources. (Also, 1 and 2 are better off with the elimination of 4, since, although one or the other must eventually transfer resources, both must transfer less with 4's elimination as a result of 3's initial threat.)

9. An equilibrium is perfect if, for each $i \in S$, no *arbitrarily small probability of others' defecting* from their equilibrium strategies yields an incentive for i to defect.

10. For any C there is an infinity of threats in T_c, including an infinity of type 1 threats (which differ only in the distribution of $r^\circ(S - C) - [R/2 - r^\circ_{\max[C]}]$ among $C - \{\max[C]\}$). We show shortly, however, that most such threats are strategically equivalent—they have the same continuation values. So for now, if we can associate a type 1 threat with C, we focus on one such threat and ignore the others that C might make. Formally, then, let $\mathbf{C}$ denote all coalitions that have type 1 threats, let $(\mathbf{r}, C)$ be a particular type 1 threat by C, and redefine the set of all threats T as $T - \cup_{C \in \mathbf{C}} [T_C - \{(r, C)\}]$. Later, we reintroduce the excluded threats to show that they leave our analysis unaffected.

11. First, with respect to T^1, $C = \{i\} \cup L_o$ has a threat satisfying condition 1 (otherwise, $L_o \in W$ and $\max[L_o] \in L$), and it follows that conditions 2 and 3 can be satisfied. With respect to condition 4, if $C' \cap C = \{j\}$, with $j \neq \max[C]$, $j \in L_o$ and $C' \notin W$. Thus, $(\mathbf{r}, C) \in T^1$. Second, with respect to T^p, it is sufficient to show that regardless of the other threats in T^p, C has a threat in T^p. From condition 1, C cannot have a threat in T^p only if $C' \subset L$ has a threat in T^p, but from condition 4 in the definition of type 1 threats, C' does not have such a threat. Hence, from condition 2 in the definition of T^p, every T^p must contain a threat by C in T^1. It follows that every $i \in S$ is in some C that has a threat in T^p. Notice that by *this argument, every* $\{i\} \cup C^* \in W$ *has a threat in* T^p, where $i \in L$ and $C^* \subsetneq L_o$.

12. Notice that it is for this reason that we select the particular threats in the preceding section to illustrate matters in the four-country case. Specifically, as our discussion of the example illustrates,

it is these threats in particular (as against (150, 150, 0, 0), (150, 0, 150, 0), and (150, 0, 0, 150)) that would be chosen as an initial threat.

13. Let $(\mathbf{r}, C) \in T^p$ and let nature order $S - C$ as O. We want to show that $S - C$ must transfer to max$[C]$. Let $j \in S - C$ be last in the order O, and consider the counter $(\mathbf{r}', C')$, $j \in C'$. Clearly, $C \cap C' \neq \varnothing$, and so we have three cases. *First,* if $i \in L_o \cap C \cap C'$, i rejects $(\mathbf{r}', C')$ since $r_i > r^\circ_i$ and, from the assumption that $(\mathbf{r}', C')$ satisfies C1 or C2, $v_i(\Gamma_{r'}) \leq r^\circ_i$. *Second,* suppose that $\max[C] \in C \cap C'$. If $\max[C'] = \max[C]$, then, since $(\mathbf{r}, C) \in T^p$, $(\mathbf{r}', C') \notin T^p$, in which case, from C1, $r_{\max[C]}$ $(= R/2) > v_i(\Gamma_{r'})$ $(< R/2)$, and max$[C]$ rejects $(\mathbf{r}', C')$. Similarly, if $\max[C'] \neq \max[C]$, max$[C]$ again rejects $(\mathbf{r}', C')$. *Third,* let $L' = \{i \in L : r_i < R/2\}$, and $C \cap C' = L'$. So, if $(\mathbf{r}', C') \in T^p$, then, from C2, $r_i > v_i(\Gamma_{r'})$ for all $i \in L'$; and each such i rejects $(\mathbf{r}', C')$. If $(\mathbf{r}', C') \notin T^p$, it satisfies C1. Letting $i^* = \min[L']$, since $C \cap C' = L'$, it must be the case that $r^\circ_{i^*} < r^\circ_{\max[S\,-\,C]}$, so from C1, $v_{i^*}(\Gamma_{r'}) < r^\circ_{i^*}$, and i^* rejects $(\mathbf{r}', C')$. Hence, the last country in O must propose a transfer; and this reasoning can be applied to the next-to-last country in O, and so forth. Thus, the only counter to $(\mathbf{r}, C)$ is a transfer to max$[C]$ by $S - C$, which establishes C2 with respect to $(\mathbf{r}, C)$.

14. Let nature order $S - C$ as O. We have three cases: *First,* $r^\circ(S - C) + r^\circ_{\max[C]} \geq R/2$ and $(\mathbf{r}, C) \notin T^1$. Let $K = \{(\mathbf{r}', C') \in T : C' \in W, C' \cap C = \{k\}, k = \max[C']\}$. Clearly, $K \neq \varnothing$. It is sufficient to show that there exists $(\mathbf{r}', C')$, $(\mathbf{r}_+, C_+) \in T^p \cap K$ such that $C' \cap S - C \neq \varnothing$ and $C_+ \cap S - C \neq \varnothing$ and $\max[C'] \neq \max[C_+]$. To establish that at least one $(\mathbf{r}', C') \in K$ is in T^p, we show first that $K \subseteq T^1$, which requires that for any $(\mathbf{r}', C') \in K$, there exists $(\mathbf{r}'', C'')$ such that $C'' \in W$, $C'' \cap C' = \{j\}$ and $j = \max[C''] \neq \max[C'] = k$. Suppose $(\mathbf{r}'', C'')$ exists and let $C'' = \{h \in S - C' : r^\circ_h < r^\circ_j\} \cup \{j\}$, where $j \in C'$. By assumption, $C'' \in W$, so, since $r^\circ_i > r^\circ_k > r^\circ_j$, $\{h \in S - C' : r^\circ_h < r^\circ_j\} \cup \{i\} \in W$. But $i \in S - C'$, which implies $\{h \in S - C' : r^\circ_h < r^\circ_j\} \cup \{i\} \subseteq S - C' \notin W$, so $(\mathbf{r}', C') \in T^1$. Let $C' = S - C + \{k\}$ with $k = \max[C']$. Since $(\mathbf{r}', C') \in T^1$, $(\mathbf{r}', C') \notin T^p$ only if there exists $C_{oo} \in W$ such that $C_{oo} \cap C' = \{k\}$, $\max[C_{oo}] = k$. However, since $r^\circ_i > r^\circ_k$ and $C_{oo} \in W$, $C_{oo} - \{k\} + \{i\} \in W$; but $C_{oo} - \{k\} + \{i\} \subseteq S - C' \notin W$, so $C_{oo} \notin W$. Hence $(\mathbf{r}', C') \in T^p$. Now substitute i for k in C' to obtain C_+. To see that C_+ has a threat $(\mathbf{r}_+, C_+) \in T^p$, notice that $(\mathbf{r}_+, C_+) \in T^1$, because the definition of T^1 concerns only those $j \in C_+$ such that $r^\circ_j < r^\circ_i$. So $(\mathbf{r}_+, C_+) \notin T^p$ only if $S - C_+ + \{i\}$ has a threat in T^p. But $S - C_+ + \{i\} = C$, and C has no threat in T^p. So $(\mathbf{r}_+, C_+) \in T^p$. Hence, there exists at least two counters in T^p to $(\mathbf{r}, C)$, namely, $(\mathbf{r}', C')$ and $(\mathbf{r}_+, C_+)$, with $\max[C'] \neq \max[C_+]$, so $v_i(\Gamma_r) < R/2$ for all $i \in L \cap C$. And since all counters are in T^p, $v_i(\Gamma_r) \leq r^\circ_i$ for all $i \in L_o \cap C$. To establish that $v_i(\Gamma_r) \leq r^\circ_i$ for all $i \in L \cap C$ for which $r^\circ_i < r^\circ_{\max[S-C]}$, we must show that no counter in T^p by any $j \in S - C$ has such an i as its maximal member. Using CC2, let $G \subseteq S - C$ and $C' = G \cup \{i\}$ with $\max[C'] = i$. If $C' \in W$, $C' - \{i\} + \{\max[S - C]\} \in W$, but $C' - \{i\} + \{\max[S - C]\} \subseteq S - C \notin W$, which is a contradiction. *Second,* $r^\circ(S - C) + r^\circ_{\max[C]} \geq R/2$ and $(\mathbf{r}, C) \in T^1$ but $(\mathbf{r}, C) \notin T^p$. Hence, there exists $(\mathbf{r}', C') \in T^p$ such that $C' = S - C + \{\max[C]\}$, $S - C' = C - \{\max[C]\}$. If nature orders $S - C$ as O, then if all counters up to the last player in O are rejected, this last player in O proposes a terminal transfer to max$[C]$ by $S - C$, which is accepted by max(C) and all members of $S - C'$. Therefore, if the next-to-last player in O proposes a threat by $\{i\} + L_o$, $i \in L$, by Lemma 1 this threat is in T^p, so it is accepted. This argument can be repeated to establish that the first player in O can propose a threat in T^p that has a positive probability of being accepted; thus, condition C1 is established. *Third,* $r^\circ(S - C) + r^\circ_{\max[C]} < R/2$. Hence, the last country in O has counters in T^p that are accepted: any counter in T^p that includes $i \in O$ must include two or more members of C, since $S - C + \{j\} \notin W$, $j \in C$ and from the assumption of risk aversion these members will prefer to freeze the system. Alternatively, if $i \in O$ offers a counter $(\mathbf{r}', C')$ not in T^p, then by the assumption of the lemma, $\mathbf{v}(\Gamma_{r'})$ satisfies C1. If no counter is available to $i \in O$, then $S - C$ is eliminated and $i \in S - C$ is indifferent as to the final allocation of its resources. So let i propose a transfer of the resources of $S - C$ in accordance with CC1. This argument applies also to the next-to-last country in O, and so on; so, in accordance with CC1, the first country in O might as well propose the transfer the last country in O would propose or a counter that satisfies C1 if such a counter exists that is accepted, in which case $v_{\max[C]}(\Gamma_r) < R/2$ and $v_i(\Gamma_r) = r^\circ_i$ for all $i \in C - \max[C]$.

15. We can now reintroduce those previously excluded threats by showing that they are strategically equivalent to threats already considered, where $(\mathbf{r}, C)$ and $(\mathbf{r}', C)$ are equivalent if $\mathbf{v}(\Gamma_r) = \mathbf{v}(\Gamma_{r'})$. Recalling that $\mathbf{C}$ denotes the coalitions with a threat in T^1, let T'_C be all the type 1 threats by C (threats that differ only by the distribution within $C - \{\max[C]\}$ of $r^\circ(S - C) - [R/2 - r^\circ_{\max[C]}]$. From the proof of Lemma 2, all such threats are strategically equivalent (since the proof of that lemma requires only that $r_i \geq r^\circ_i$ for all $i \in C - \{\max[C]\}$). Thus, Γ's strategic structure is unaffected if we select one element of T'_C as representative of the set and "discard" the rest. There remains the threats in $T''_C = T_C - T'_C$, such as, with four countries ((130, 0, 90, 80), {1, 3, 4}). In this example, the only coalition with a threat that might be viable against ((130, 0, 90, 80), {1, 3, 4}) is {1, 2}, such as ((150, 150, 0, 0, {1, 2}), because C1 does not exclude the possibility that $v_1(\Gamma_{(150,150,0,0)}) > 130$. If this is true,

$\mathbf{v}(\Gamma_{(130,0,90,80)}) = \mathbf{v}(\Gamma_{(150,150,0,0)})$. However, no simultaneity confounds the determination of $\mathbf{v}(\Gamma_{(150,150,0,0)})$, since this value is not a function of $\mathbf{v}(\Gamma_{(130,0,90,80)})$: if $i \in S$, whenever it is otherwise indifferent, chooses a counter in T^p, $\mathbf{v}(\Gamma_{(130,0,90,80)})$ has no bearing on $\mathbf{v}(\Gamma_{(150,150,0,0)})$. More generally, for any $(\mathbf{r}, C)$, $C \in \mathbf{C}$, no threat by another $C' \in \mathbf{C}$ is viable against $(\mathbf{r}, C)$. If no threat is viable against $(\mathbf{r}, C)$, $(\mathbf{r}, C)$ is strategically equivalent to the threat in T^p by C; thus, we can discard $(\mathbf{r}, C)$. If there exists $(\mathbf{r}', C')$, $C' \notin \mathbf{C}$, that is viable against $(\mathbf{r}, C)$, $\mathbf{v}(\Gamma_r)$ satisfies C1. In either event $(\mathbf{r}, C)$ satisfies C1 or C2.

16. On the other hand, if $j \in C \cap L$ is risk-acceptant or if the gain from defecting is sufficiently great, all countries, including i, know this beforehand and i will not propose an initial threat with $j \in C$. Moreover, Lemma 1 establishes that for any $i \in L$, there exists a threat in T^p such that C contains only i and members of L_o. Thus, if j would reject i's proposal, i can find a threat that is accepted.

17. That 1's and 3's responses do not depend on the possibility that the reallocation of $r°_5$ alters Γ's strategic character among $\{1, 2, 3, 4\}$ follows from two readily established facts. First, letting E denote the set of essential countries and E_o denote the set of inessential ones, no reallocation of $r°_j$, $j \in E_o$, renders $i \in E$ inessential or $k \in E_o$ essential. Second, if $i \in E$ but $r°_1 + r°_i < R/2$, no reallocation of $r°(E_o)$ renders $\{1, i\} \in W$. Hence, if $i \in E$ cannot initially buy stability, it cannot do so after an inessential country is eliminated.

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THE MORAL FORCE OF POLITICAL OBLIGATIONS

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Political obligations vary in force. Though we have strong obligations to obey certain laws, our obligations to obey others appear to be considerably weaker. Because the weakness of the obligations to obey certain laws has been employed as an argument against the existence of general prima facie political obligations, an adequate theory of political obligation must account for this. By employing the obligation to keep promises as a model, I sort out the factors that contribute to the force of prima facie political obligations. Their varying force can be explained according to a general theory of political obligation founded on the principle of fairness.

A number of scholars have argued in recent years that there are no prima facie political obligations. Typically, these scholars proceed by puncturing the traditional theories of obligation. When none is found to hold, the existence of prima facie political obligations is placed in doubt (Pateman 1979; Raz 1979, chap. 12; Raz 1986; Simmons 1979; Smith 1973). In addition to arguments along these lines, certain scholars have found more direct evidence in the very nature of political obligations. I will focus on arguments of this sort. In particular, I will look into the fact that political obligations appear to vary in force and the problem that this poses for theories of political obligation. I will then attempt to show how a theory of political obligation rooted in the principle of fairness is able to account for this.

The Varying Force of Political Obligations

We can begin by pointing out two main features of prima facie political obligations. First, *obligations* (as the term is commonly used) are construed on the model of promises. They are distinguished from other moral requirements because of their specificity: they are created by specific voluntary actions or performances, owed to particular individuals, and discharged through the performance of specific actions. However, in discussions of *political obligation* a wider usage is preferable. The current demand for an adequate theory of political obligation would be satisfied if one could bring forth good reasons for complying with the injunctions of one's government, whether or not these reasons stem from specific voluntary actions. Thus, I will discuss political obligation in the broad sense.[1] Second, *prima facie obligations* (as the term has been used since its introduction by Ross) are defeasible. Obligations, like other moral requirements, are subject to being overridden by conflicting requirements (Ross 1930, 18–20). Under certain circumstances promise p can be overridden by considerations q and in fact ought not to be complied with. However, in such a case p does not cease either to be a promise or to have generated an obligation.[2] By saying that p generates a *prima* *facie obligation,* we distinguish this obligation from what we can call *conclusive obligations,* which express what we ought to do when all relevant moral considerations have been taken into account.

The language of prima facie obligations is helpful in reminding us that political obligations are of limited force. But this realization raises another problem, namely, to ensure that they are sufficiently strong. Because modern states exercise their authority through the rule of law, questions of political obligation can be construed as questions about the obligation to obey the law. We can assume that an adequate account of political obligation will explain our obligation to obey all laws that are passed by legitimate authorities. In order to distinguish political obligations per se from other moral requirements to obey the law, we can say that A's political obligation is his or her obligation to obey the law *because it has been passed by the proper authorities* rather than for other reasons. The immediate problem is that our obligations to obey certain laws are weak. We can get a rough idea of the force of a particular obligation by considering the censure that would follow the discovery of its violation. According to this standard, the obligation to stop at a red light on a deserted road in the middle of the night is not very strong, as A would probably face only mild censure (if that) if he were discovered not to have stopped. On similar grounds we would say that the obligation to stay within the speed limit is also frequently weak.

The fact that certain of our political obligations are so weak has been employed as an argument against the existence of prima facie political obligations. According to M.B.E. Smith, in order for theories of obligation to be worthy of serious consideration, the obligations that they establish must be substantial (1973, 970–76).[3] However, as our red light example shows, the obligation to obey certain laws does not meet this test. Smith suggests, then, that even if the existence of a prima facie obligation to obey the law could be established, it is not clear that it would merit much attention.

While Smith's approach indicates that political obligations are not sufficiently strong, a different analysis raises a problem of the opposite sort. In arguing for the moral authority of law (which I view as coextensive with a prima facie obligation to obey the law), John Finnis appeals to the wide range of benefits that the legal system confers and to their great complexity. These considerations establish a strong obligation to obey all laws, as presumably, all laws are implicated in the supply of benefits. In Finnis's words, "The law presents itself as a seamless web" (1984, 120). However, if the law actually were a seamless web so that our failure to obey any given law affected all laws, our obligations to obey all laws would be of significant force. But this conclusion conflicts with the fact that obligations attached to particular laws, for instance, the traffic example, are of only slight force.

An obvious way around the arguments of both Smith and Finnis is to suggest that political obligations vary in force. Though some are weak and so not worthy of much consideration, others are stronger and more important. To the extent that the varying force of obligations has been noted, it appears to have been used as an argument against the existence of general political obligations (Raz 1984, 145–49). But this can be shown to be consistent with at least one theory of political obligation.

Intuitively, it seems clear that our obligations to obey different laws are of different moral force. Legal scholars frequently distinguish between actions that are inherently wrong (*mala in se)* and actions that are wrong only because they are illegal *(mala prohibita).* There are obvious moral reasons not to perform actions of the first sort, especially actions that directly harm other people. If it is against the law to kill other people and Jane kills Dick, she would obviously face strong censure. It is also clear that violations of laws of this sort would meet with censure of varying intensities. Jane's condemnation for murdering Dick would be far greater than for shoplifting. In these cases, however, it is not clear how much of the censure would stem from Jane's violation of her obligation to obey the law and how much from committing acts that are inherently wrong.

The force of Jane's obligation to obey the law (because it is the law) can be seen more clearly in cases of the second kind.[4] Acts that if not for legal prohibitions would not be wrong include violations of various traffic laws, (such as the red light example) and certain tax laws. Now, violations of different laws of this sort will also give rise to censure of varying intensities, though it is more difficult to explain where this variation comes from. As noted above, minor traffic violations would meet with little condemnation. But other acts would give rise to strong censure even though they cause negligible harm at most. The U.S. government has a budget of hundreds of billions of dollars. A few thousand dollars more or less from Jane would make a negligible or imperceptible difference. Yet the discovery that Jane had not paid her income taxes would likely lead to widespread censure, whereas the discovery that she had violated minor traffic laws would probably be ignored. Thus, it must be explained why some violations of laws are regarded as more severe than others even though their consequences appear to be similar.

It strikes me that an adequate theory of political obligation should be expected to explain how obligations to obey different laws vary in force. If theory X construes all obligations as of the force of minor traffic regulations, the resulting obligations would appear to be too weak to account for more onerous political requirements, like the requirement to pay one's taxes. On the other hand, if theory Y construes obligations along the lines of tax laws, the resulting obligations would appear to be too strong to account for the limited force of minor traffic laws. To be consistent with our intuitions an account should present obligations of varying intensities.

The Force of Promissory Obligations

On an intuitive level it appears that the strength of political obligations should depend largely on the consequences of obeying or disobeying different laws. In general, the rightness or wrongfulness of any action will be judged largely in terms of its consequences (however these are determined). Act-utilitarians, of course, employ this standard exclusively. As we saw in the last section, Smith employs a consequentialist analysis in examining political obligations. Because violations of minor traffic regulations are liable to have only slight consequences, he concludes that prima facie political obligations are of little concern (1973, 970–76). But critics of act-utilitarianism have shown that factors other than consequences should be brought to bear in assessing the rightness of actions and that act-utilitarians have especially severe problems in explaining obligations (Brock 1973; Griffin 1982). For instance, there are clear cases in which promises should be complied with even if breaking them would have more beneficial results. Thus, though analysis in terms of consequences can help to explain how political obligations vary in force, other factors should be invoked as well.

We can gain some insight into the range of factors at work in political obligations by examining the factors that contribute to the moral forces of promises. Before proceeding, I should make clear that my analysis of promises does not rest on any specific moral theory and should be consistent with different construals of the moral basis for the obligation to keep them.[5] One view in conflict with my analysis is act-utilitarianism, due to its problems explaining cases in which promises should be kept, though the consequences of breaking them would be better. Thus, in addition to the consequences of keeping or breaking a given promise, something of its moral force appears to derive from the promise itself.

The significance of the act of promising—and of similar performances in regard to other obligations—has been analyzed by H. L. A. Hart. According to Hart, in addition to stemming from specific performances and being owed to specific people, promises, like other obligations, are characterized by "independence of content." The obligation to keep a promise does not arise from the nature of the promised action but from the promise itself (Hart 1958, 102; cf. Brandt 1964, 386-87). If James promises Henry that he will do p and p is independently the sort of action that he should perform, James's particular obligation to do p does not stem from its independent moral desirability. Actions q, r, s, and so on, can be equally desirable, yet James will have no obligations to do them over and above his general moral duty to perform morally desirable actions. The fact that James promises to do p separates it from the general class of morally desirable actions and creates an obligation in regard to it. Contrariwise, if t is a morally wrong action, James's promise still creates an obligation for him to do it—though there are complexities here that will be explored below. In this case the obligation is obviously independent of the nature of the promised action.

Though independence of content appears to capture important features of promises, Hart's account can be elaborated upon. Hart's discussion of independence of content appears to be in reference to the *existence* of obligations. A promise is a certain kind of performance that creates a particular obligation, without regard to what the promise is about. The obligation is created by the performance and does not otherwise exist. But the *force* of a promise is affected by what is promised. To capture this additional aspect of promises (and other obligations) we can distinguish between the performance that creates an obligation and the content of the obligation. In the case of a promise, as long as A utters some appropriate verbal formula (or otherwise invokes the conventions of promising that exist in A's society) A can make a promise to B regardless of what is promised. In this sense independence of content holds.

But the content of the obligation affects its force. If Jack promises to do something that is morally wrong, under many circumstances his promise should not be complied with. He would be subject to greater censure if he committed a severe crime after promising to do so than if he broke his promise and did not commit it. In a case of this sort, it appears that the obligation created by the promise is overridden by the wrongfulness of what is promised. In order to facilitate discussion, we can refer to the *value* of an obligation. In assessing reasons for and against complying with a given obligation, those in favor of compliance can be said to have positive value, and those against, negative value. I will assume that the values of different aspects of an obligation can be added and subtracted to yield its cumulative value, or the overall strength of the considerations in favor of complying (or not complying) with it. By analogy with the concept of "truth-value," we can say that if an obligation should be complied with when all relevant moral concerns have been taken into account, it has positive value. An obligation that is overridden by conflicting factors can be said to have negative value. Employing this terminology, we can say that in certain cases carrying out a promise can have negative value, because the negative value of the promised action outweighs the value of the transaction through which the promise comes into existence.

In any given case it appears that the value of an obligation depends on a complex interplay between the performance through which it is created and its content. Content here centers largely on the consequences of performing or not performing the promised action. The content of a promise has positive value if the consequences of keeping it will be beneficial and negative value if they will be harmful.[6] Though it is difficult to determine the precise values of the different aspects of a given promise, a rough approximation is provided by the simple formula that the positive value created by the transaction through which the promise comes into existence will be increased by the positive value of beneficial consequences and decreased (and possibly overridden) by the negative value of harmful consequences. Thus, if keeping some promise would give rise to great good, A's obligation to comply with it would be stronger than if the consequences were less beneficial. Along similar lines, if A promises B to commit a crime, then even though the promise itself has some moral force, its value will be counteracted by the negative value of the crime. In many cases these negative factors will override the positive value of the promise itself, and it should not be complied with.

Confirmation of the role that consequences play is that a promise's value will vary as we vary the consequences of complying with or breaking it. Let us assume that Susan is about to have a baby. She already has a young child, June, who must be cared for when she goes into the hospital to give birth. Bob promises Susan that he will take care of June. If Bob breaks this promise (which we will assume he does for no pressing reason), his behavior would be clearly wrong, and he would be subject to censure. As the consequences of his breaking the promise varied, the degree of his wrongdoing would vary accordingly. If Bob called Susan as she was beginning to go into labor to inform her that he was no longer available, the censure would be strong, especially if his behavior seriously inconvenienced her at this difficult time. Bob's violation of his promise under such circumstances would be considerably worse than if he notified Susan in advance—and the farther in advance, the less the censure. If he notified Susan sufficiently far in advance to allow her easily to make other arrangements, his wrong would be relatively slight. As this example shows, as the consequences of noncompliance with a given promise change, the force of the obligation to keep the promise changes as well.

Additional factors play a role in determining the strength of promises. Though all transactions performed in accordance with the conventions of A's society will constitute promises that are of some moral force, the value of promises will vary as the transactions themselves vary. A solemn oath is of greater force than a promise made more casually. The increased force that particular verbal formulas create is clearly understood by children who demand that certain promises be reinforced with "Cross your heart and hope to die" and the like. Along similar lines, promises made in particular circumstances are of special force. The most notable examples are deathbed promises, which are widely viewed as especially binding. Of course, there are cases in which even solemn oaths and deathbed promises should not be complied with, for instance if compliance would be inherently wrong. But because transactions of these sorts imbue the promised action with considerable value, the conditions under which they should not be kept will be more difficult to satisfy than for other promises.

Because promising is a complex institution, the elements that we have discussed could well interact differently in different cases. It is not possible to say in the abstract exactly how different elements should be weighed in particular cases. But the analysis here should contribute to the assessment of specific promises in making clear the factors involved. It seems safe to say that in general the value of a promise will depend on the interaction of (1) elements bearing on the transaction through which it arises (e.g., its solemnity and its circumstances) and (2) its content, expressed in terms of the consequences of compliance or noncompliance.

Generalized Consequence Analysis

If we construe political obligations on the model of promises, we must locate a feature corresponding to the performance through which a promise comes into existence. The force of the political obligation could then be assessed by combining the value of the performance and the value of complying with a given law, expressed in terms of the consequences of compliance or noncompliance. This sort of analysis would account for the fact that political obligations vary in strength, as obedience to different laws involves different consequences. However, there are problems in assessing the content of political obligations in terms of consequences, while it is also difficult to identify aspects of political obligations corresponding to the performances that give rise to promises.

According to one account of political obligations, the difficulties in construing them on the model of promises are relatively slight. According to theories of obligation based on consent, A's obligation to obey the law stems from an actual promise, whether express or tacit. On this account, the factors discussed in the last section would seem clearly relevant in assessing the force of political obligations. But because theories of consent have been severely criticized by many scholars and are generally viewed as fatally flawed, they can be set aside.[7]

At first sight it might appear that a consequentialist theory of obligation, for example, one based on act-utilitarianism, would be more promising. Roughly, on this analysis, the force of an obligation to obey a specific law would depend on the consequences of obedience or disobedience. This might explain why relatively trivial laws (such as that in the red light example) are of slight force, while more important laws (such as those concerned with paying taxes) possess greater force. Because we do not require that an account of political obligation be based on obligations in the strict sense and so do not require the existence of specific performances, a consequentialist account would not be vulnerable on this ground. However, consequentialist theories encounter grave difficulties in certain of the most important cases with which political philosophers deal, namely those involving coordinating the activities of large numbers of citizens. Many of the crucial benefits that governments provide are of this sort. Clear examples are national defense, the rule of law, and protection from natural disasters and threats to public health. Benefits such as these, commonly referred to as *public goods*, are characterized by *nonexcludability*. It is difficult to provide them to certain members of a given society while denying them to other members. Under most circumstances they must be provided to all members if they are provided at all.[8] Other important benefits that governments provide are also public goods, for instance, many public health measures. In addition, though such components of a societal infrastructure as roads, bridges, harbors, and airports are not necessarily public goods since their use could be limited to particular individuals (e.g., those who pay user fees), such limitations would frequently involve severe practical difficulties, so that they too can be regarded as public goods.

A second crucial feature of the benefits under discussion is that their provision generally requires the cooperation of large numbers of people. The rule of law in society X requires general adherence to X's legal code. Though the cooperation of all X-ites is not necessary, social order depends on the acquiescence of a high percentage of X-ites. Though the precise percentage needed (commonly referred to as a *threshold)* is difficult to specify, at some point a widespread lack of cooperation would undermine the sanctity of the legal code, thereby threatening the security of all. The case is similar with national defense and other governmental services financed with tax revenues. Though universal compliance with tax laws is not necessary, at some point widespread failure to obey them would jeopardize the government's ability to provide important services, thereby threatening societal functioning and the well-being of all inhabitants.

Because important benefits that governments provide (1) are public goods and (2) require large-scale cooperation, their provision is beset by a troubling pattern of analysis, commonly referred' to as the *prisoner's dilemma.*[9] In many cases it is difficult to justify requiring a given individual (or a relatively small group of individuals) to cooperate in the provision of such benefits on consequentialist grounds. For instance, by paying her taxes Shelly contributes imperceptibly to the public good. If she did not pay, the amount she contributes would scarcely be missed. However, Shelly's tax payments make a considerable difference to her. If she did not pay she would have more money to lavish on herself and her friends, perhaps to contribute to worthy causes. Since her tax money could make a perceptible difference if it were spent in these ways, it is not clear that the consequences of her not paying would actually be worse for society than if she paid. Similarly, by serving in the armed forces Sean would contribute imperceptibly to the security of X. But because he would be far happier if he did not serve and his contribution would not be missed, it is not clear that his military service would actually render society better off. Similar examples could be multiplied. What they have in common is the fact that in a large society in which the cooperation of most but not all members is required to provide some important public good, it is difficult to demonstrate that society actually benefits from the cooperation of any given individual.[10]

The prisoner's dilemma is damaging to consequentialist accounts of political obligation. On consequentialist grounds, A should obey the law because society benefits from his doing so. But there is a range of important cases in which it is not clear that his obedience is actually beneficial. Consequentialism encounters similar difficulties in explaining how political obligations vary in force. We have noted that the obligation to pay one's taxes appears to be far stronger than the obligation to stop at a red light on a deserted road. But in a large society, under many circumstances the consequences of disobeying the two laws would be similar. Because Shelly's tax payments would make no perceptible difference to her government's ability to pay its bills, it is not clear why her failure to pay is more harmful than her failure to obey minor traffic laws.

A more promising approach relies on a particular variant of consequentialism. Though there would be no detectable consequences if Sean refused to pay his taxes or serve in the armed forces or if he violated various other laws, the situation would be markedly different if *everyone* (or an unspecified, large number of X-ites, to which, for convenience, I will refer as *everyone* or *all X-ites)* behaved similarly. I will refer to an analysis that relies on the consequences of large numbers of individuals behaving in a certain way as a *gener-*

alized consequence analysis.

In speaking of generalized consequence analysis, I must make clear that I am not arguing according to a strictly consequentialist moral view, such as *utilitarian generalization.* Roughly, according to utilitarian generalization, a given act is right under some circumstances if the consequences of its performance by everyone (who is able to do so) would be at least as good as those of the general performance of any other act. Utilitarian generalization is a controversial moral view that has been severely criticized; the claims that I make do not rest on its distinctive features.[11] As was true of my discussion of obligations to keep promises, my account of the force of obligations to obey the law does not rest on a specific moral theory. The generalized consequence analysis is necessary to capture one aspect of our intuitions about political obligations, namely, that their force correlates (roughly) with the generalized consequences of obeying or disobeying given laws. Regardless of the specific moral theory to which we refer to explain this, it should not strike us as surprising. We can assume that laws are ordinarily made to serve the public interest, with general obedience as their intended object. Explaining our intuitions about the force of political obligations is logically distinct from explaining the moral principle (or principles) from which the obligations stem, though an adequate account of the origin of obligations should contribute to explaining the variance in their force.

The generalized consequence analysis is immediately more plausible than a view that relies on the consequences of individual actions. We have seen that the obligation to pay one's taxes is stronger than that connected with some minor traffic law. This variance is well explained by a generalized consequence analysis because the consequences of everyone's refusing to pay their taxes would be far more severe than everyone's violating minor traffic laws. Similarly, in times of national emergency, the consequences of wholesale avoidance of conscription laws would be disastrous. Thus, it is not surprising that individuals who are convicted of desertion or of dodging the draft are subject to severe censure. Though assessments along these lines are necessarily rough, I believe that further examples would support a rough correlation between the force of the obligation to obey a given law and the generalized consequences of obeying or disobeying it.

A generalized consequence analysis is also supported by connections that are frequently drawn between the behavior of A and of all other X-ites. When we consider cases of disobeying laws, a common reproach that A is likely to meet if he is found not to have paid his taxes is, "What if everyone did the same?"[12] In the first recorded set of arguments for political obligation in the Western tradition, in Plato's *Crito*, the personified Laws of Athens reply along these lines to Socrates' proposed willingness to escape from prison and avoid his death sentence: "Tell me, Socrates, what are you intending to do? Do you not, by this action you are attempting, intend to destroy us, the laws, and indeed the whole city, as far as you are concerned? Or do you think it is possible for a city not to be destroyed if the verdicts of its courts have no force but are nullified and set at naught by private individuals?" (50a–b [Grube 1978]). Thus, Socrates should not escape because his doing so would destroy the laws of Athens.

Though cases like these are frequently encountered, they raise a severe problem for a general consequence analysis. To the Laws' question "What if everyone did the same?" there is an obvious response: there is virtually no chance that everyone will do so. In itself, Socrates' escaping would have little effect on Athenian laws. Though the laws would be destroyed if everyone disobeyed them, the requisite connection between Socrates' disobedience and everyone else's has not been established. In many cases A's failure to obey a certain law would have no detectable effect on whether other individuals (or at least a sufficient number to have detectable societal effects) would obey. In a large society there are many cases in which A's actions would not influence those of other people, but people would still condemn A's failure to obey the law.

Though actual connections between A's behavior and that of the requisite number of X-ites are not always easy to draw and utilitarian generalizations does not seem an acceptable approach, the generalized consequence test can be supported in other ways.[13] In recent years theories of political obligation based on the principle of *fairness* have received a good deal of attention. The principle was originally formulated by Hart in 1955 and developed by other thinkers, most notably John Rawls (Hart 1955; Klosko 1987a; Rawls 1964). The principle centers on the idea of the mutuality of restrictions: individuals who benefit from the cooperative efforts of others have an obligation to cooperate as well. As scholars have shown, the principle of fairness rests on a more general moral principle, which David Lyons refers to as "the just distribution of benefits and burdens" (1965, 164). According to Rawls, "We are not to gain from the cooperative labors of others without doing our fair share" (1971, 112; also Arneson 1982 and Ewing 1953). A clear example is national defense. If A benefits from the protection his fellow citizens provide, he has an obligation to cooperate in their efforts. He can be obligated to serve in the armed forces or to help finance them with tax payments or both.

The principle of fairness provides the requisite connection between the behavior of A and other X-ites. According to an alternative formulation of the principle, presented by Rawls, the principle "requires one to abstain from an advantage that cannot be distributed fairly to those whose efforts have made it possible" (1964, 17). National defense and other similar benefits are necessary for the well-being of all X-ites, A included. Because such goods require large-scale cooperation and all benefit from their provision, A would behave unfairly in receiving the benefits of others' cooperative activity without cooperating himself. If the cooperation of many but not all is required to provide some good, why should A, rather than other X-ites, be free of the burdens of cooperation unless there is some morally relevant difference between A and them?

Content Value

I have argued elsewhere that the principle of fairness can support a workable general theory of political obligation. Arguments for this theory (which I will refer to as *fairness theory)* need not be reviewed here.[14] More important for our immediate concerns is the fact that fairness theory is able to explain how political obligations vary in force. Because great precision concerning the force of political obligations is not easily attained, our account must be somewhat rough. But the presentation here should be able to cover our intuitions about important features of political obligations.

In assessing the strength of political obligations, two factors should be taken into consideration. I will call these *content value* and *institutional value*. Content value is concerned with the importance of the benefits that stem from particular laws. Though the importance of different benefits cannot be identified precisely, we can divide the benefits supplied by governments into rough classes, obligations corresponding to which appear to vary in force. The first class is comprised of benefits essential to individuals' well-being. Examples of these have been noted: national defense, the rule of law, protection from a hostile environment. For example, in the case of national defense the cooperation of large numbers of X-ites is required for the benefit to be produced. If Karl receives the benefit without bearing his share of the burden, his behavior would be clearly unfair, and he would be subject to strong censure. As noted, during times of national emergency (e.g., in the U.S. early in World War II), individuals who were believed to have illegally avoided the draft, deserted, or taken illegal liberties with the country's wartime economic measures were harshly criticized.[15] If we employ the severity of such censure as a measure, we should conclude that the obligations to obey these laws are of considerable moral force. Similarly, the rule of law is necessary to protect individuals from various sources of harm. Individuals who violate the law and thereby threaten the security of their fellow citizens are also harshly condemned. Clearly, there are strong obligations to obey a range of laws bearing directly on personal security, though it is not easy to distinguish the components of these obligations that stem from immediate harms that are committed from more strictly political obligations, obligations to obey laws because they are laws.[16]

Of course, in addition to defending their populations, governments perform a much wider range of services. As noted above, the government in country X is responsible for providing and maintaining an infrastructure of roads, bridges, tunnels, and harbors. It regulates communication and education, provides sewers and sanitation, works to promote economic stability, and cares for the percentage of the population that is unable to care for itself. The population of X will contribute to government's provision of these extensive services in various ways, most notably by providing tax revenues and by following the laws (e.g., traffic regulations) that are necessary to coordinate their actions with their fellows.

The benefits represented by this range of governmental services vary enormously in importance. Those that any given program provides citizen A can be quite different from those enjoyed by other citizens. Because of this wide range of benefits and differences in how they fall out on different individuals, it seems unlikely that we would be able to specify the moral force of specific obligations to comply with specific programs. However, it seems likely that there is a rough correspondence between the importance of the overall benefit that a given law produces and the force of the obligation to obey it. Thus, the obligation to obey minor traffic laws is less strong than that to pay one's taxes, which is in turn less strong than that to serve in the armed forces in times of dire need. Again, as noted in the last section, the benefits in such cases should be assessed in terms of generalized consequences. In itself A's avoidance of the draft laws may do no more harm to society than his or her failure to stop at a red light on a deserted road. But the consequences of generalizing the former sort of behavior would be far worse than the latter. Similarly, we can make rough gradations within the range of governmental services. Thus, we distinguish between significant and insignificant traffic violations. Going very fast on a busy highway or in a residential neighborhood is a more serious offense than failing to stop at a red light on a deserted road, whether or not harmful consequences result from specific instances of each kind of act. Generalizing the former sort of behavior would have more severe consequences than generalizing the latter.

It seems, then, that there is a rough correspondence between the force of the obligation to obey a given law and the benefits the law provides, assessed in terms of generalized consequences. In contributing to the force of a given obligation, the *im-*

portance of these benefits corresponds roughly to the *content* of promises.

Institutional Value

In another sense laws should be obeyed simply because they are laws, with reference to a different aspect of their specific consequences. In addition to content value, obligations to obey laws also have institutional value. We can refer to the benefits received from the rule of law and the existence of a working social decision process as *institutional goods.*[17] Because the benefits that Karl receives from institutional goods depend on widespread willingness to comply with the law, it follows from the principle of fairness that he, too, has an obligation to cooperate. He should not profit from the sacrifices of other people if the advantages of noncooperation cannot be extended to them as well. We can call the argument that grounds A's obligation to comply with a given law in A's need for the rule of law the *institutional argument.*

According to the institutional argument, Karl's obligation to obey the law stems from the benefits he receives from the rule of law and the means it provides of peacefully settling disputes. These benefits are of fundamental importance, necessary for the continued existence of society and its members. Karl's obligation to obey stems from the corrosive effects of disobedience. Because the rule of law requires general adherence, individuals who lightly set aside laws with which they disagree undermine the social fabric.

The importance of the rule of law gives individuals an additional reason to obey specific laws, the force of which is derived from a second set of generalized consequences. Let us assume that A confronts the question whether to obey some tax law T of society X. The force of this obligation reflects the consequences that generalized disobedience of T would have upon X. In addition to the specific subject matter with which it is concerned, a given law is a component of the legal system. Because the rule of law in society X requires general obedience to its laws, T included, general obedience or disobedience of T will affect the rule of law in society as a whole.[18]

The size and complexity of a modern legal system render the institutional force of obligations to obey the law complex. These obligations appear to differ in regard to different kinds of laws pertaining to different social functions. Though we need not sort out numerous kinds of laws, one distinction is important. In a basic sense the rule of law is indispensable to the maintenance of society and so to the well-being of its members. Laws that are important in this regard include prohibitions against physically harming other people, taking their property, and defrauding them. We can refer to the direct contribution of the rule of law to personal security as its core or essential function. The laws under consideration here are central to that minimal social order necessary for personal security—for life so "as not to be weary of it," to use Hobbes's words (1968, 192). Since A undoubtedly requires the rule of law in this sense, A would be unfair not to obey the range of laws in question.

The obligation to obey the law encompasses other laws as well. Laws that fall outside this core range should also be supported because this contributes to the core functions. There is an empirical assumption here, but one that is virtually truistic among political philosophers:[19] widespread adherence to the law promotes further adherence, while widespread disobedience has the opposite effect. If individuals develop a habit of disobedience in regard to relatively minor matters, that habit will extend to more important matters, while a habit of obedience in minor matters will strengthen obedience in major matters. Accordingly, compliance with laws falling outside the core range is indirectly good in itself, because it works to strengthen the core function of the rule of law.

It seems clear that the institutional component of the obligation to obey specific laws will vary in force, depending on the contribution that generalized adherence to a given law makes to the rule of law in society.[20] Thus, laws falling in the core range will have greater institutional force than laws that contribute only indirectly to the core function. In many cases laws that have considerable institutional force will also have significant content value, as general disobedience would lead to serious direct harm. The obligations to obey such laws will therefore be doubly strong.

The fact that the societies discussed here are large makes matters more complex. In a large society, Kate's disobedience of a given law, whether major or minor, is unlikely to have an appreciable effect on the degree of adherence to the rule of law throughout society as a whole. If she disobeys minor laws, her resulting habit of disobedience is unlikely to have detectable social effects. According to the principle of fairness, however, the actual consequences of a given act are not the sole concern. As long as the rule of law exists in her society and nonadherence by others would weaken their commitment to it, Kate benefits from their obedience; and the advantages of her noncompliance cannot be extended to them. Thus, Kate should not disobey minor laws; because general disobedience of even minor laws would undermine the rule of law throughout society, it would be unfair of her to disobey them.

The discussion of institutional value up to this point has omitted various complicating factors. Different kinds of exceptional cases should be considered, however briefly. First, because the principle of fairness traces obligations to the receipt of benefits, in circumstances under which laws are widely disobeyed so that Kate does not benefit from the rule of law, the institutional value of particular laws should be low or nonexistent. As Hobbes says, "He that should be modest, and tractable, and performe all he promises, in such time, and place, where no man els should do so, should but make himselfe a prey to others, and procure his own certain ruine" (1968, 215). Not only would it be foolish for Kate to obey when all others do not, but it would be unfair to require her to do so. Along similar lines, the institutional value of political obligations is subject to being overridden by other moral considerations. For example, assume that Lisa's country, Y, is grievously unjust, along the lines of Nazi Germany. In this case, the fact that the rule of law exists and so provides her with important benefits would create an obligation for Lisa to obey Y's laws. But it is likely that this obligation would be overridden by the moral requirement not to commit unjust actions (or support their perpetration), just as the obligation to keep a promise can be overridden by the injustice of what is promised.

A more difficult range of cases centers on the fact that particular laws appear to be without institutional value even though (1) the rule of law exists in the legal system in question and (2) the system as a whole is acceptably just. In a complex society it is likely that many laws will be instituted that individuals do not feel they should obey. Many laws are in fact widely disobeyed, and it may seem unclear that there is an obligation to obey them. Undoubtedly, beyond a certain point, specific laws that are unfair or unjust lose their binding force. Especially important for our purposes, it is not clear that laws that cannot be shown to serve real or legitimate social purposes or those that actually detract from public welfare, should be obeyed. However, despite appearances to the contrary, these laws have positive institutional value, though this can be overridden by their negative content value. When some law is widely believed to be detrimental to society, people can generally feel that it should not be obeyed despite the implications that disobeying it might have for the rule of law throughout society. In a complex legal system many sorts of laws lose their rational justifications over time if they ever had them. Frequently, such laws pass out of use and cease to be enforced. Examples are laws against witchcraft and similar practices, still on the books in various countries. Similarly, blue laws can pass out of social fashion before they are officially repealed. In many cases useless laws can be identified by the fact that they are violated openly, without strong disapproval. For instance, certain traffic laws, such as the 55 miles per hour speed limit, are routinely disobeyed. Proof that the benefits of noncompliance with these laws can be generalized without undue harm to society is the fact that nonadherence is widespread.

The variables in these cases are difficult to sort out, but it appears that in many cases the widespread belief that a given law has negative content value will lead to the widespread belief that it can be rightfully disobeyed. Speed limit laws are especially clear instances here, as are also laws forbidding various social practices, for example, certain sexual relations among consenting adults. Public attitudes indicate general disdain for speed limits. The fact that those who are caught speeding and ticketed are often met with sympathy rather than censure is strong evidence that (under many circumstances) the laws in question are widely believed not to serve legitimate social purposes. Under other circumstances, however, speed limits serve clear and important purposes and are regarded differently. When individuals are known to go very fast, say 100 miles per hour on busy highways, they constitute a danger and are accordingly subject to censure. Or if individuals speed in residential neighborhoods, especially where children are playing, they too become dangers and will meet disapproval. Thus, there are circumstances in which these laws are clearly important, as is indicated by public reaction. If, under other circumstances, a given law actually is useless and widely disobeyed, modifying it would serve an important social purpose in preventing a habit of disobedience from developing in its regard.

Innumerable complexities bearing on the institutional component of the obligation to obey different laws cannot be discussed here. But to hazard a few generalizations by way of summarizing, it seems that even in questionable cases, individuals can be presumed to have prima facie obligations to obey the law because it is the law. In most cases, one can assume that this obligation is of significant moral force. To the extent that disobedience of any law undermines the general fabric of the rule of law on which the welfare of all depends, individuals who disobey are subject to condemnation. We have seen, however, that there are exceptions to this rule—that when a given law is unjust or detracts from the public good, the obligation to obey can be overridden.

It seems that the force of the obligation to obey a given law can be assessed after the model of the obligation to keep a promise. We have seen that the force of the obligation to keep a promise is comprised of the value of the performance that gives rise to the promise, combined with the value of the consequences of keeping or breaking it. As we have seen, the force of the obligation to obey a given law depends on two factors. We have, first, the generalized consequences of obedience to the law, its content value. Second is one particular generalized consequence of obedience or disobedience, that bearing on the law's contribution to the rule of law throughout society, its institutional value. Once again, in the abstract it is difficult to specify the force of a given law. However, the analysis here should be helpful in indicating the factors to be taken into account in assessing any given case.

It appears that all laws have positive institutional value, though some laws have greater institutional value than others, as generalized obedience to them is more central to maintenance of the rule of law in society. In assessing the moral force of a given law, institutional value should be combined with content value. As noted, institutional value will vary in relation to the contribution that general obedience to a given law makes to the rule of law. Content value will also vary in strength. As the consequences of general obedience to different laws become increasingly important, the value of their corresponding obligations will increase. But as we have seen, in some cases the consequences of general adherence to a given law will be either negligible or actually harmful. In certain cases negative content value can negate or actually override positive institutional value, and the obligation to obey certain laws will be of little or no force. Such laws are often recognized by the fact that they are violated openly, with little condemnation. It seems that laws of this sort should be stricken from the books, because of the potentially corrosive effects of their open violation on the rule of law throughout society.

Conclusion

I have discussed two different ways in which the moral force of political obligations is commonly evaluated. Smith assesses political obligations in terms of their consequences and notes that the obligations to obey certain laws appear to be weak. Finnis discusses the interwoven complexity of legal systems. Viewing the law as a seamless web, he believes that there is a significant obligation to obey all laws. It seems that both of these approaches are partially correct; my argument confirms both up to a point. We have seen that the obligation to obey a specific law is in part a function of its consequences (generalized consequences). We have also seen important institutional considerations that support obligations to obey all laws. In regard to these factors, the law is a seamless web; within limits, disobedience of any law undermines the habit of obedience on which the entire legal system rests.

I believe that our intuitions about political obligation support both a presumption that a law should be obeyed because it is a law—a presumption that is subject to being overridden by the negative value of specific laws—and the view that obligations to obey different laws differ significantly in force. We have seen that the force of the obligation to obey a specific law can be interpreted as a combination of the law's content and institutional values and that fairness theory accounts for both of these components. The fact that fairness theory is able to ground our intuitions about the varying force of political obligations is a significant consideration in favor of its claim to support a workable theory of political obligation.

Notes

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1. For the concept of obligation, see Brandt 1964; for its wider use in discussions of political obligation, see Simmons 1979, chap. 1 and Smith 1973, 950–52.

2. Cf. the language of Ross 1930, 18–20; for problems with his language, see Searle 1978.

3. Smith's additional argument concerning the additive force of the obligation to obey the law (1973, 970–71) will not be discussed here; but see below, n. 16.

4. See below, n. 16.

5. For different analyses, see Prichard 1968, and the exchange between Raz (1972) and MacCormick (1972).

6. To keep discussion reasonably simple, I do not

discuss additional factors relevant to the rightness or wrongfulness of promises, as, for instance, if fulfilling a given promise would cause one to violate certain individuals' rights or would be inherently wrong for other reasons. Obviously, if rights would be violated by Adam's fulfilling some promise or if it would be inherently wrong for him to keep it for some other reason, this would tell strongly against his obligation to keep it.

7. Though I believe that consent theory is irretrievably flawed, it would still merit consideration if all other theories of political obligation were equally or more defective. Although the matter cannot be discussed here in detail, I believe this is not the case. Because the principle of fairness roots obligations in the receipt of benefits and does not require specific voluntary performances, it is able to ground a theory of political obligation that possesses the advantages of consent theory without its disadvantages; see Klosko 1987a. An excellent discussion of consent theory and its flaws is found in Simmons 1979, chaps. 3–4.

8. Public goods (as the term is generally used) are also characterized by nonrival consumption, that is, that one individual's consumption of a given good will not affect the amount available for others. This condition is not directly relevant to my concerns and so is not discussed.

9. An important recent discussion is Parfit 1984, part 1. Though the cases under discussion can be referred to as prisoner's dilemma cases, they do not fall under the prisoner's dilemma in the technical sense. In prisoner's dilemma cases the different participants have different maximands, whereas in the cases under discussion we can assume common interest; see Regan 1980, 62–63.

10. This line of argument is disputed by Parfit (1984, chap. 3); his view is criticized in Klosko 1990b.

11. For criticism of utilitarian generalization, see Lyons 1965 and Gruzalski 1982.

12. See Broad 1915–16, Ewing 1953, and Strang 1960. For evidence of connections between A's political obligations and the behavior of other people, see Klosko 1987b.

13. I do not consider various versions of Kantianism because of the objectionable metaphysical commitments they generally involve.

14. See Klosko 1987a, which responds to important criticisms of the principle of fairness, including those of Nozick 1974, 90–95 and Simmons 1979, chap. 5. (See also Klosko 1987b in criticism of Simmons.) Klosko 1990a responds to Simmons' (1987) criticisms of Klosko 1987a.

15. Individuals who are believed to violate the spirit, as opposed to the letter, of such laws are also censured. It seems, however, that those who are believed to violate the letter (as well as the spirit) are criticized more harshly, as is indicated by public attitudes in regard to the recent insider trading cases on Wall Street or the tax law conviction of Leona Helmsley.

16. The generalized consequence test does not seem clearly to apply to certain actions that violate laws, especially criminal laws prohibiting actions that entail direct and immediate harms (such as laws against murder, robbery, and rape). It should be noted, however, that the test does seem to apply in certain cases (e.g., white-collar crime, stealing from the rich, and robbing banks) in which the actions under consideration are strongly condemned even though it is not clear that they cause immediate harm. Because acts such as murder and rape are immediately harmful (and so, clearly wrong), the test is not normally appealed to. But this does not indicate that it does not identify an *additional* respect in which the acts are wrong. As is seen in Smith's example of the moral difference between killing 1000 people and killing 1001 people (1973, 970, n. 37), our intuitions are not always reliable in cases in which we identify additional respects in which acts that are obviously reprehensible are wrong. It should also be noted that criminal laws perform important coordination functions in society so that general adherence plays an obvious role in maintaining the social fabric; see Boardman 1987.

17. It is not necessary for us to sort out different aspects of the rule of law and the values associated with each. On the formal conception of the rule of law and its benefits (which are distinct from the protection of persons from coercive interference by others), see Raz 1979, chap. 11. I attempt to capture aspects of the rule of law in this sense in discussing a working decision procedure, which provides society with stable, predictable rules governing social interaction.

18. Though my overall argument does not require any particular account of the obligation to keep promises, the analogy between this obligation and that to obey the law is more clearly seen if we accept (for the sake of argument) an account that roots the obligation to keep promises on concerns of fairness; for this analysis, see Rawls 1971, sec. 52. According to this construction, if A breaks a promise to B, his wrong is twofold: (1) the damage done to B, who relied on A to keep the promise and (2) the unfairness of breaking a promise. According to a fairness account, the practice of promising rests on general adherence to its rules. By making and then breaking a promise, A acts unfairly; he takes an advantage that cannot be extended to those whose efforts make it possible. If we accept this construction, the force of promissory obligations involves factors very similar to those seen in political obligations, as discussed in this section.

19. Aristotle, *Politics*, 2. 13–14; Thomas Aquinas, *Summa theologica*, 1–12. 97. 2 (in D'Entreves 1973, 72–73).

20. In contrast to Smith (1973), who believes that obligations to obey laws because they are laws are of

only slight moral force, the argument here suggests that the force of such obligations will vary. Some laws have slight force, while others have much more, depending upon the contribution that general adherence to a given law makes to the rule of law in society.

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MAKING THE CITY SAFE FOR PHILOSOPHY: *NICOMACHEAN ETHICS,* BOOK 10

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The perennially problematic relationship between philosophy and politics, though recognized as an important theme in the Platonic corpus, is virtually ignored in the writings of his most famous student. This is not due to the absence of the problem but the deftness of Aristotle's treatment. Attentiveness to both the requirements of moral-political life and the nature of philosophy gives rise to the rhetorical design of the Nicomachean Ethics. *In the final book of the* Ethics *Aristotle establishes the value of philosophy by placing his argument within a broader context that reveals to what extent moral and intellectual excellence can be regarded as similar and even complementary. Without actually denying the existence of a fundamental tension between the requirements of philosophy and civic virtue, Aristotle succeeds in winning an at-least-partial acceptance of philosophy on the part of those who are (or will be) most responsible for the welfare of the city.*

The conclusion of the *Nicomachean Ethics* is well known not only among students of philosophy but, more broadly, by virtually all who consider themselves to be students of Western culture: complete or perfect happiness is to be found in the philosophic activity of contemplation, the practice of moral virtue is happy in a secondary degree (1177a 12–18, 1178a 9–10).[1] It may be that the success of Aristotle's argument in the *Ethics* as a whole (and Book 10 in particular) has obscured both the force of this jarring conclusion and the full measure of Aristotle's accomplishment. To the extent that the *Ethics* has become one of the classic texts of the Western tradition, it has succeeded in eliciting an acknowledgment on the part of nonphilosphers of the dignity and importance of the philosophic life.[2]

That this was no easy task is suggested by recalling the animosity directed against philosophers (especially political philosophers) expressed in the writings that antedate the *Ethics.* The suspect character of philosophy as it was perceived by the most spirited and ambitious contemporaries of Socrates is reflected in the plays of Aristophanes and the dialogues of Plato. Aristophanes' *Clouds,* whatever else it may contain, is clearly intended to warn his fellow citizens about the dangerously corrosive effect of philosophy on healthy political life. One might think also of Callicles' stinging description of the politically incapacitating consequences of a serious preoccupation with philosophy (*Gorgias* 484c–486d). Not only does the hostility of the city and of those most responsible for its welfare come to a head in the trial and execution of Socrates, but Plato's presentation of Socrates suggests that this animosity is in some sense reciprocal. Socrates tells his jurors that it is impossible for him or anyone seriously concerned with justice to survive in the political arena (*Apology* 31d–32e), a sentiment echoed elsewhere and often in the Platonic dialogues (see *Gorgias* 521a–22e; *Republic* 496a–e; *Meno* 92a–95a). I recall these well-known facts because it is against this backdrop that the unique character of Aristotle's political writing comes most fully into view. Although Aristotle, like his teacher before him, maintains the superiority of the philosophic life, his political treatises contain nothing like this overt warfare; they seem rather to mute the tension between philosophers and those who are primarily concerned about and responsible for the good of the city.

Whereas the priority that Aristotle assigns to philosophic contemplation over moral virtue is obvious to all students of the *Ethics*, the precise nature of the relationship between them has fired a great deal of debate. Despite the obvious importance of this issue, scholars remain sharply divided. The deepest gulf is between those who maintain that Aristotle argues for some combination of moral and intellectual excellence and those who maintain that his recommendation of contemplative excellence is separable from, and perhaps even incompatible with, his teaching on moral excellence. With varying degrees of difference, the former position is argued by Clark (1975), Hardie (1968, 12–27; 1979), Lord (1982), and Rorty (1980, 377–94). The latter problem is raised by Ackrill (1974) and Rodier (1957, 213–17). It is given its sharpest expression by Cooper (1975, revised in 1989), Nagel (1972) and Wilkes (1978). Mara (1987) seeks to articulate a middle ground.

At the very least, this continuing controversy about the exact relationship between moral and intellectual virtue in the *Ethics* suggests a certain ambiguity in Aristotle's treatment. I wish to argue that this is a deliberate ambiguity shaped by specific *apologetic* concerns which, at least in part, are responsible for the extraordinary influence of Aristotle's political writings among those who do not primarily or essentially regard themselves as philosophers.

I shall bring to light the apologetic dimension of Aristotle's presentation of philosophy by focusing on the artfulness of his argument in the final book of the *Ethics*.[3] Aristotle's argument proceeds through four themes: pleasure, happiness, the best way of life, and the turn to politics. I shall trace the rhetorical design of Book 10 as a whole by considering each of these themes in turn.

It should be noted that *rhetorical design* refers to the specifically Aristotelian understanding of rhetoric. In contrast to Socrates, who debunks rhetoric as a kind of "knack" that aims at flattery (*Gorgias* 462b–65e), Aristotle maintains that rhetoric is an "art" that seeks to understand the various means of persuasion (*Rhetoric* 1355b27–35). According to Aristotle, rhetoric can be used to reveal the truth and is characterized by attentiveness to both the character and limits of the audience being addressed in any given situation (*Rhetoric* 1356a1–36; see Nichols 1987 and Arnhart 1981, 3–53). This double concern for both truth and the particular audience to which the truth is addressed gives rise to the rhetorical dimension of Aristotle's argument in the *Ethics*.

Aristotle's Edifying Account of Pleasure

The Horizon of Book 10

Scholars have long noted discrepancies in Aristotle's double treatment of pleasure (7. 11–14, 10. 1–5) but have neglected to observe the very different horizons within which Aristotle offers these two accounts. The prevailing interpretation, articulated most fully by Festugière (1960), minimizes the differences that distinguish these accounts, viewing them instead as different stages in Aristotle's effort to develop a single unified teaching on pleasure (see Gauthier and Jolif 1970, 783; Hardie 1968, 295; Leiberg 1958, 2–15; Ross 1974, 228). A notable exception to this approach is provided by Owen, who writes, "Traditionally the question has been whether the two accounts are too divergent to be compatible. I hope to show that they are too divergent to be incompatible" (1977, 93). Although Owen's thesis has met with only limited success in displacing the understanding of the problem advanced by Festugière, it has the merit of showing that Aristotle's account in Book 7 contains a consistent and positive teaching of its own. Further, it leads Owen (and invites his readers) to grapple with the fundamental differences that distinguish this account from the one in Book 10.

Aristotle begins his initial treatment of pleasure in Book 7 by recommending it as a subject worthy of contemplation for political philosophers (1152b1–3). This contrasts sharply with the didactic concern that guides his second consideration in Book 10; Aristotle reintroduces the subject of pleasure because it bears on the education of the young toward moral virtue and the attainment of the happiness that is proper to the practice of virtue (1172a19–25).

Aristotle's approach to the question of pleasure in Book 10 is consistent with his approach to the study of moral virtue as a whole. He indicates two extreme positions with a view to directing his readers toward a (virtuous) mean. Some argue that pleasure is the good, others that it is entirely base. Among those who adopt the latter position, Aristotle points out that some really believe this to be true, whereas others maintain this position for salutary reasons. They argue that the natural inclination toward pleasure among human beings makes it advisable to point most persons in the opposite direction with the hope that this will bring them closer to the mean.

Aristotle takes issue with this approach, but not with its didactic purpose (1172a33–b8). Whenever a particular theory or position appears to be inconsistent with action, it is either despised or dismissed. Aristotle points out that the theories are dismissed not because they are untrue but because, for most, actions speak louder than words. Hence, to the extent that it is possible to bring theory into harmony with practice, the theories in question will prove more useful for the conduct of life. This practical aim characterizes Aristotle's consideration of pleasure in Book 10; it is intended to "encourage (protrepō) those who are capable of understanding ethical arguments to live better lives.

It is important to realize that this is not the only way that Aristotle approaches the subject of pleasure in the *Ethics*. Although a full consideration of Aristotle's initial treatment of pleasure in Book 7 extends beyond my scope here, it is useful to recall a few surprising, even disturbing, features of that treatment.[4] The consideration of pleasure directed especially to actual or potential political philosophers (1152b1–3) maintains that pleasure has the status of an end (1153a7–17) and, even more surprisingly, that nothing prevents pleasure from being the supreme good (1153b1–54a7). The argument concludes by identifying the most godlike activity with the enjoyment of pleasure (1154b25–28).

Without going so far as to suggest that Book 7 advocates a certain kind of hedonism, it is sufficient to observe that Aristotle's initial treatment of pleasure reveals a perspective that is quite alien to the teaching on moral virtue for which the *Ethics* is rightly famous. By way of contrast, Aristotle's final consideration of pleasure is explicitly subordinated to the moral-political horizon that dominates his study as a whole; in Book 10 Aristotle adopts and continues to clarify the perspective of his decent—but not necessarily philosophic—readers.

Aristotle's New Description of Pleasure

In Book 7 Aristotle defines pleasure as "unimpeded activity" and leaves open the possibility that pleasure is both the highest good and something divine. In Book 10 Aristotle offers not so much a redefinition as a new description of pleasure. Pleasure "perfects" or "completes" (*teleō*) activity (1174b23). In this new view, pleasure belongs to, or depends on, activity.

Because activities differ in kind, the pleasures that belong to and augment them can be differentiated (1175a 21–76a3). By arguing that activity, not pleasure, is fundamental, Aristotle can not only distinguish various pleasures but do so in a salutary way by determining the moral worth of the activities to which the various pleasures correspond. On the basis of this distinction, Aristotle is able to rank or evaluate the bewildering variety of pleasures sought and experienced by human beings. In all cases, Aristotle writes, the thing is as it appears to the morally serious man (*ho spoudaios)* (1176a 15–16). Moreover, if we wish to know what constitutes the distinctively human pleasure or pleasures, we should look to the best activity or activities of "the perfect and blessed man (*tou teleiou kai makariou andros*). All other pleasures sought by human beings are "human" in secondary and even more tenuous ways, depending on the activities from which they arise (1176a24–29).

More precisely, Aristotle maintains that the "perfect and blessed man" reveals the pleasure or pleasures that are human in the most authoritative (*kuriōs*) sense and that other pleasures are human in a secondary (*deuterōs*) sense and still others are even more remotely (*pollostōs*) connected with distinctively human activity (1176a26–29). Although *deuterōs kai pollostōs* is often taken as a single idiomatic expression in Aristotle, it is helpful to call attention to the distinct meanings of these adverbs. Taken literally, this passage introduces the threefold hierarchy (made explicit in the following consideration of happiness) that, I shall argue, provides the rhetorical framework for Aristotle's argument in Book 10.

Studied Ambiguity

We should not allow a certain ambiguity in Aristotle's argument to escape our notice: Who precisely is the morally serious man? Is Aristotle thinking of the magnanimous man or the philosopher when he speaks of "the perfect and blessed man?" Despite the obvious importance of this question, Aristotle has not at this point provided his readers with a definitive answer. He does not even call attention to the question; rather, he seems intent on retaining a certain ambiguity concerning the identity of the individual in question. Indeed, such ambiguity is not without practical effect, since it is likely to elicit greater openness to Aristotle's argument on the part of the majority of his morally serious readers who are able and likely to read it as an acknowledgment that they provide the standard for all who fall below them.

Happiness and the Pleasures of the Powerful

The Unsettling Example of the Powerful

In Book 1, Aristotle presents happiness as the starting point and end of the study of ethics. Whereas Book 1 provided an outline or sketch of the relation between happiness and moral virtue, Book 10 addresses this subject in a thematic way. Aristotle begins by recapitulating his initial assertions about happiness from Book 1 (1176a30–76b9). Happiness is not a disposition but some form of activity that is chosen for its own sake and is lacking in nothing. Aristotle points out that this way of describing happiness is also believed to describe the nature of virtuous actions, since noble and virtuous deeds are desirable for their own sake.

This implicit identification of happiness and moral virtue is taken over from Book 1 and constitutes the perspective that Aristotle has, with a few notable exceptions, adopted and clarified throughout his study. Only in Book 10 does he address in a serious way a problem that challenges the special or authoritative status such a view confers on morally serious persons. The pleasures of play also appear to be desirable for their own sake (1176b 9–17).[5] So simple an assertion is not so simply dismissed. Not only are such amusements thought by many (*hoi polloi*) to constitute happiness; but also and especially, absolute sovereigns (*turannoi*) are seen to devote themselves to such pastimes. The life-styles of the rich and famous undermine the salutary belief that happiness is to be found in the morally serious activities of decent persons.[6]

The Authoritative Standard of the Morally Serious Person

Aristotle addresses this problem by seeking to establish the unique dignity of moral virtue despite the weight of actual political practice (1176b17–77a11).

His arguments in this section combine the natural standard of pleasure with the standard provided by decent opinion. He does this in two ways. First, Aristotle relies on his earlier distinction (pleasure derives from activity) to rule out a priori the possibility that pleasure could provide an independent standard for human conduct. Hence, the most pure (*eilikrinēs*) and liberal (*eleutherios*) pleasures arise from the most morally serious activities of virtue (*aretō*) and intelligence (*nous*) (1176b18–28).

It should be noted, however, that the claim that pleasure could provide an independent standard for judging human action is reflected in the pastime activities of the powerful. Indeed, Aristotle explicitly raises the crucial theoretical issue whether life is for the sake of pleasure (1175a 18–19). However, the question is left unresolved; Aristotle simply sets it aside as inappropriate in the present context (see 1153b1–54a7).

Second, by including both virtue and intelligence within the single category of activities that give rise to pure and liberal pleasures, Aristotle does not call attention to the ambiguous meaning of *virtue* (*aretō*), a word that embraces both moral and intellectual excellence. He likewise minimizes the extent to which the different activities falling within the categories of *aretō* and *nous* (or the pleasures arising from them) can lead to radically different ways of life. Aristotle is elsewhere more forthcoming about the difference between the moral-political and contemplative ways of life. One might consider, for example, Aristotle's differentiation of three ways of life at the outset of the *Ethics* (1095b14–96a5), his subsequent discussion of prudence and wisdom and the different exemplars provided for each (see 1140a24–b11, 1141a20–b14), and the emphasis he places on the difference between the practical and theoretical ways of life at the beginning of his discussion of education in the best regime (see, esp. *Politics* 1324a25–34).

There is, of course, some warrant for Aristotle's conflation, since, as he has already indicated in Book 6, the life of moral virtue also involves reason. Aristotle's argument in the present context is consistent with his earlier lack of precision in this matter. Instead of indicating that the activities of virtue and intelligence can lead to two different ways of life, he uses both as attributes to describe the activity of the "morally serious person," an expression that is itself laden with ambiguity.

Aristotle's argument in this chapter counters the widespread view that happiness consists in the pleasures of play with the authoritative standard of morally serious persons who live in accordance with virtue. The philosopher and the nonphilosopher are united in opposing the authority of sovereigns who, by their example, teach that happiness consists in the enjoyment of the pleasures of play. Morally decent persons possess greater authority on this issue than the rich and famous because they experience pleasures that are purer, more liberal, more serious, and, in general, more specifically human than those typically indulged in by the powerful.

Happiness and the Best Way of Life

Ambiguity Dispelled

It is only after establishing this alliance among morally serious persons that Aristotle chooses to clarify the exact relationship between moral and intellectual excellence as it bears on the all-important question of happiness.[7] His jarring conclusion: complete happiness is to be found in the contemplative activity of philosophers, the practice of moral virtue accompanied by happiness in a secondary degree (1177a12–18, 1178a9–10).

Aristotle supports this conclusion with six arguments (1177a18–b26)

1. Contemplation is the best (*kratistē*) activity because it involves the best thing in us.
2. It is the most continuous activity in which human beings can engage.
3. It is held to contain pleasures of marvelous purity and permanence.
4. It is the most self-sufficient activity, for the wise man need not depend on others in order to contemplate.
5. It is loved for its own sake and produces no result beyond itself.
6. It is an activity of leisure par excellence.

On the basis of these six arguments, Aristotle concludes that contemplation is the highest activity in accordance with virtue and, consequently, that complete human happiness consists in this activity.

This argument confronts all of Aristotle's readers in an unambiguous way with a truth that many of them would necessarily find disconcerting. However, unlike Platonic presentations of similar teachings in the mouth of Socrates, which are invariably met by anger or ridicule on the part of Socrates' interlocutors, Aristotle has reason to expect that his presentation of this teaching will be neither dismissed nor ridiculed by the majority of his nonphilosophic readers. Not only has Aristotle painstakingly clarified the horizon of moral virtue from the point of view of moral virtue in the first half of the *Ethics*,[8] but (as we shall see) even this most explicit teaching on the superiority of the philosophic life is offered within a broader context that emphasizes the similar and even complementary character of moral and intellectual virtue.

Similar Ways of Life

The similarity between philosophic and moral excellence is suggested by the particular arguments Aristotle has used to maintain the superior happiness of the contemplative life. All six of these arguments apply, to some extent at least, to moral excellence as well. It is possible to argue that a life characterized by the practice of moral virtue is superior to, and happier than, a life given over to the enjoyment of bodily pleasures and amusements (the example of sovereigns notwithstanding) for precisely the reasons that Aristotle has offered to establish the ultimate superiority of the philosophic life.

Indeed, in the course of his study of politics these arguments, or variants of them, are in fact applied by Aristotle to moral excellence. With respect to the six arguments enumerated above, one should recall the following Aristotelian assertions:

1. Justice is considered the best (*kratisē*) of the virtues (1129b27–28).
2. Friendship (based on the practice of moral virtue) facilitates the continuous activity which characterizes happiness (1170a5–11).
3. Pure and liberal pleasures accompany virtue and intelligence (1176b17–21) or, more particularly, the moral virtue of generosity (1120a26–29).
4. The magnanimous man aims at self-sufficiency (1125a11–12).
5. Noble actions are desirable for their own sake (e.g., 1120a23–25, 1122b6–7, 1176b6–9).
6. Justice and moderation are activities appropriate to leisure (*Politics* 1334a11–35).

Aristotle's argument in Book 10 maintains that the philosophic life is happier than the moral life not because it is radically different from it but because it offers to a greater and more perfect degree the very things that decent persons both seek and enjoy for themselves. It is within the broader context of several arguments bearing on both philosophic and moral excellence that Aristotle presents his most explicit teaching on the superiority of the philosophic life.

Complementary Ways of Life

Aristotle continues his argument on the superiority of the philosophic life (1178a9–b7) by explaining that moral virtue is reduced to a secondary status because it is an activity limited by a need for, and dependence on, others. By way of contrast, the happiness of contemplation requires less "external equipment"—a term that, in Aristotle's usage, includes other human beings. In fact, Aristotle suggests that the kind of equipment necessary for doing virtuous deeds, especially great deeds, might in some cases be a hindrance to contemplation (1178b1–5).

If this argument is more forthcoming about a dissimilarity and possible tension between the moral and contemplative ways of life, Aristotle immediately restrains this candor by asserting that the philosopher, since he lives in society, also chooses to engage in virtuous actions (1178b5–7). If, as Aristotle has just argued, morally decent persons should look up to the philosopher as the exemplar of the best way of life, he now indicates that the philosopher is not—or at least should not be—indifferent to the moral-political concerns that are the special preoccupation of the city and those who bear primary responsibility for its welfare. The philosopher is not indifferent to the constantly shifting sands of moral-political life because he is human (*anthrōpeuesthai*) and for that reason needs to live among other human beings (1178b7; see 1154b20–31). Aristotle's manner of argument emphasizes not conflict but complementarity in the relation between theoretical and moral virtue.

This argument is, however, double-edged. Whereas it is reassuring to learn that philosophers are or should be concerned about moral virtue, the argument simultaneously raises a question about their motivation. The philosopher does not choose to do virtuous deeds because they are the best one can do or because they give rise to a certain type of happiness. Rather, it is because he lives among human beings that he practices moral virtue, for this enables him to live among decent human beings. For the philosopher, moral virtue is not an end in itself but rather a means to his own activity.

A harsh expression of this so-called "intellectualist argument" is found in *Magna*

Moralia, where moral virtue is reduced to the status of a household manager who attends to daily necessities so that the lord of the house might enjoy the freedom and leisure to engage in philosophic thought (1198b12–20; see *Eudemian Ethics* 1249b5–25; also Nagel 1972, 258). The difference between this account and the one given in the *Nicomachean Ethics* is revealing. Although both accounts clearly affirm the superiority of the philosophic life, in the *Ethics* moral virtue is said to be subordinate to, but never the servant of, philosophic contemplation. Whereas the latter view suggests that moral virtue is ultimately devoid of intrinsic dignity, the argument of the *Ethics* is distinctive precisely for its insistence on an independent status for moral virtue. Indeed, Aristotle's often-repeated assertion that moral acts are undertaken for their own sake comes as close as any single line to constituting a refrain for the entire book. In the *Ethics* Aristotle subordinates the life of moral virtue to the philosophic way of life while at the same time retaining a sense of its importance. The life of moral virtue (unlike the life of slavery) does result in substantial human happiness (1178a20–22). Nevertheless, a still greater possibility exists for those who are able to direct their activities to the almost superhuman happiness of philosophic contemplation.

Although moral virtue is originally demoted to a secondary status because, unlike contemplation, it is dependent on external goods for its exercise, Aristotle's final argument on this subject diminishes (without actually denying) this difference (1178b33–79a9). Since human nature is not self-sufficient with respect to contemplation, even philosophers must concern themselves with external well-being. However, as the life of philosophy shows, happiness does not require an abundance of external goods. In this respect it is possible for the practitioner of moral virtue to imitate the philosopher, since one can also undertake virtuous actions with modest resources; it is possible to do noble deeds without being a ruler of land and sea (1179a4–5). Indeed, Aristotle observes that private citizens, philosophers and nonphilosophers alike, are more likely to act in accord with the requirements of virtue than those in positions of political authority.

In this final evaluation of the moral life, Aristotle counters the unsettling example of sovereigns who believe that happiness depends on an abundance of external goods with the standard of happiness revealed in the philosophic life. Aristotle bolsters his conclusion by drawing from the sayings of Solon and Anaxagoras—a wise statesman and a philosopher (1179a 9–17). Both citations give added authority to his salutary teaching that contrary to what most believe, happiness is not to be found in the excesses enjoyed by tyrants but rather in imitating, as much as possible, the materially unencumbered activity of philosophers.[9]

Through a philosophic study of ethics Aristotle directs his readers to real, rather than illusory, happiness. Insofar as decent persons are more concerned with practicing moral virtue than amassing an abundance of external goods, they are able to experience something of the godlike happiness of the philosopher. On the other hand, Aristotle's study teaches philosophers that they need to take seriously the life of moral virtue. This is due primarily to the greater opportunity to philosophize afforded by the company of decent human beings, especially those who, through Aristotle's guidance, have come to some appreciation for the value and dignity of the philosophic life.

Persistent Tension

Although Aristotle does not emphasize the dissimilarity and tension between moral and contemplative virtue in Book 10, neither does he deny it. The persistence of this tension is reflected nowhere more clearly than in his description of the gods (1178b7–22).[10] The gods do not need external goods, and all forms of moral virtue seem small and unworthy of them; contemplation is the only activity that can reasonably be attributed to them. The harsh character of this description of the gods—one that echoes an earlier account in Book 7 (1154b26–31)—lies in the implication of divine indifference to questions of moral virtue and to the changing realm of human affairs in its entirety.[11]

From this lofty vantage point Aristotle argues that happiness is coextensive in its range with contemplation (1178b28–32). Insofar as the philosopher is able to approximate godlike happiness, Aristotle's argument provides further evidence for the view that the serious philosophic concern with moral virtue is in tension with moral virtue itself. In light of the essentially political character of human beings, human imitation of divine indifference toward moral virtue is reprehensible; however, the appropriate human approximation of divine indifference expresses itself in the cultivation of moral virtue as a means to the almost divine activity of contemplation, not as an end in itself.

This teaching on the relationship between human and divine activity is at odds with the final argument of the *Ethics* on this subject (1179a24–29). Aristotle maintains that if, as is generally held, the gods exercise some concern over human affairs, they are likely to benefit those who most love and honor godlike excellence in themselves. The premise of this argument flatly contradicts Aristotle's previous depiction of the gods; divine indifference to moral virtue has given way to divine concern for "correct and noble" (*orthōs kai kalōs*) human actions.

Greater attentiveness to the text reveals that the contradiction does not lie in Aristotle's thinking, since this final argument about the gods is explicitly drawn from a pervasive and decent opinion rather than from Aristotle himself. Thus, without actually retracting his theoretical speculations about divine activity, Aristotle allows them to be in some way *eclipsed* by a concluding and morally edifying argument about the gods. What is the reader to make of these two competing views of divine activity? Whereas the most attentive readers might be more impressed by the distance between human and divine concerns and what that implies about the best way of life for a human being, Aristotle's final reference to the gods in the *Ethics* mutes the harshness of his earlier considerations (in Books 7 and 10 both) by encouraging all his readers to live in accordance with the best thing in them and to revere rather than demean those who most embody the peculiar excellence of philosophic contemplation.

The Turn to Politics

Moral Nobility and the City

In the concluding chapter of the *Ethics* Aristotle returns to the theme that has implicitly guided his study as a whole and explicitly guided his consideration in Book 10—the moral education of the young. Aristotle observes that although speech is sufficient to encourage the most generous youths to pursue a life of moral nobility (*kalokagathia*), speech by itself is unlikely to awaken a similar desire in the majority of human beings (1179b7–10). Since most are ruled by passions, compulsion and punishment are more effective than the appeal to reason and the noble (1180a4–5). Hence, there arises a need for laws, which take their bearings from moral nobility but are also backed by the strong arm of the city. The salutary application of force is necessary not only for the city (in that it establishes the rudiments of political order) but also and especially for individual citizens, since it encourages them to live in accordance with the best thing in them.

If the argument of the preceding chapters demotes the life of moral virtue in light of the most perfectly happy life of philosophy, the concluding chapter of the *Ethics* draws on the broader context within which Aristotle has established that argument, namely, the extent to which these two ways of life are similar and complementary. Aristotle argues that those whose lives are susceptible to the influence of reason *and* the noble provide the authoritative standard for the mass of mankind whose lives are circumscribed by passion. Aristotle concludes his study of ethics and introduces the study of politics in a way that allows the resplendent character of moral virtue to stand together with philosophic contemplation as the authoritative standard for the laws of the city. Aristotle's conclusion emphasizes the dignity of a life characterized by the practice of moral virtue, its salutary importance for politics, and the need that all those who bear primary responsibility for the city have for the philosopher, who—in the person of Aristotle—promises to help his readers gain a better understanding of the crucial relationship between virtue and actual political practice in the second half of his "philosophy concerning the human things."

Conclusion

The aim of this essay has been to bring to light the rhetorical art of the concluding book of the *Ethics*. The particular character of Aristotle's well-known conclusion concerning the superiority of the philosophic life is shaped by a less-appreciated concern for the character and limits of his nonphilosophic readers. Although it would be wrong to conclude from this fact that Aristotle considers these arguments to be false, it would be equally misleading to identify them in an unqualified way with Aristotle's own view of the subject. Aristotle's rhetorical presentation of philosophy in the *Ethics* succeeds in muting—without actually denying—a fundamental tension between philosophy and politics. As such, an awareness of the apologetic dimension of the *Ethics* is essential for assessing the teaching of this book and its proper place within Aristotle's thought as a whole. Careful study of the concluding book of the *Ethics* provides the reader with an example of the Aristotelian understanding of the art of rhetoric, an art that is used not to obscure the nature of philosophy but rather to reveal substantial, if incomplete, truths about it. If, as Aristotle maintains, Alcidamas' rhetorical claim that philosophy should be considered "a bulwark of the laws" is too exaggerated to be credible (*Rhetoric* 1406b5–15), Aristotle's rhetorical art is calculated to win an at-least-partial acceptance of philosophy on the part of those who are or will be most responsible for directing the affairs of the city.

Notes

1. Numerical citations are taken from the Oxford edition and refer to the *Nicomachean Ethics* unless otherwise indicated. Translations, though indebted to Rackham (1926) and Irwin (1985), are my own.

2. I assume not only that the majority of those who read and benefit from their study of the *Ethics* are not philosophers, but that the *Ethics* was written with this audience in mind. This premise is suggested by the fact that Aristotle's study does not offer a philosophic defense for the claims of morality but rather presupposes a prior acceptance of those claims on the part of its readers (e.g., 1095a30–b8, 1103b21–34). As such, the *Ethics* is addressed especially to the better sort of persons referred to in classical literature as "gentlemen." In Aristotle's usage, *gentleman* (*kalogagathos*) is a term of distinction connoting both social-political status and a certain level of moral excellence. The gentleman is a citizen in the fullest and best sense of the word, one who embodies the highest aims of the polis. See *Eudemian Ethics* 1248b8–49a18; also de Sainte Croix 1972, 371–76; Strauss 1953, 142–43; Strauss 1977, 21, 25–28; Urmson 1988, 3.

3. Scholars have generally neglected the apologetic dimension of Aristotle's political writings despite the persistence of stock charges against philosophers during Aristotle's lifetime. After the death of Alexander the Great in 323, Eurymedon in-

dicted Aristotle for impiety (Diogenes Laertius, *Lives* 5.5). Aristotle decided to leave Athens before the matter came to trial, lest (as one tradition reports it) he gave Athenians a second opportunity to sin against philosophy.

4. For a thematic analysis of this question and defense of the view suggested here, see Tessitore 1989.

5. Aristotle considers a similar problem in Book 1, namely, that pleasure (especially physical pleasure) appears to constitute happiness for human beings. In that context, however, he simply dismisses this view with the kind of noble disdain that one would expect of the morally serious person, maintaining that such a life is equally well suited to cattle (1095b14–22; see 1109b7–12).

6. The morally debilitating effect of the example of the powerful appears to motivate Polus's attack on Socrates in the *Gorgias*. Polus maintains that everyone, including Socrates, consciously or unconsciously envies the powerful (468e, 470e–71e).

7. Aristotle is of course aware of the pedagogical importance of ordering an argument correctly. Although there is a scholarly tradition of dividing and rearranging Aristotle's books, recent scholarship has tended to emphasize the integrity of the *Ethics* as a whole. Typical in this regard is Rorty, who writes, "Even if the book is a thing composed of threads and patches, the organization of those threads and patches composes a perfectly coherent pattern" (1980, 3). On the organic development of arguments in the *Ethics*, see Jaffa 1952, 48–49 and Faulkner 1972, 85, n. 6.

8. Whereas Plato's *Republic* is an attempt to defend the goodness of justice for the one who practices it, the *Ethics* seeks to clarify the nature and limits of justice for those who are already convinced of its goodness. Stated more generally, Plato consistently addresses the question, Why be virtuous? This question is never *directly* raised in the *Ethics*, where Aristltle adopts, rather than challenges, the perspective of the morally decent person.

9. Although the peak moral virtues of magnificence and magnanimity require an abundance of resources, both material and political, Aristotle includes less splendid and less political versions of these dispositions in his account of moral virtue. Generosity and a nameless virtue regarding honor require only modest resources (see esp., 1125b1–9). Whereas the former two virtues may prove to be a hindrance to contemplation, this is not clearly the case with respect to the latter.

10. This is the third of three striking indications in Book 10 of a tension between moral and intellectual excellence. In each case Aristotle pierces the horizon that circumscribes his inquiry in Book 10 and then covers it over again. They are (1) the unanswered question whether life is for the sake of pleasure (1175a18–19, 19–21); (2) the suggestion that the pursuit or practice of the greatest and most noble moral virtues impedes the highest human excellence (1178b 1–5, 5–7, 1178b33–79a9); and (3) the apparently insignificant character of moral virtue in light of divine activity (1178b7–23, 1179a24–29).

11. The harsh character of Aristotle's account of divine activity in Book 10 is even more apparent when it is read in light of Aristotle's earlier account in Book 7. Book 7 concludes with the assertion that god enjoys a single simple pleasure always, a description of divine activity that is explicitly contrasted with the changing, and therefore necessarily imperfect, realm of human activity (1154b26–31; see *Metaphysics* 1072b14–31; 1074b15–34).

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THE POLITICAL PERFORMANCE OF THIRD WORLD GOVERNMENTS AND THE DEBT CRISIS

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The developing countries' suspension of payments on their external debt is as much a consequence of the political weakness of their governments and the excessive politicization of their economic policies as it is a result of unfavorable structural changes in the international economy. Differences in debtor governments' political performances are treated as an explicit variable rather than as residuals to an economic explanation in estimating the probability of developing countries' suspending their external debt service payments. Using a logit model, I analyze fifty-eight developing countries for the years 1970–1984. The results show that political capacity can be decisive in corrrectly predicting the probability of a government's suspending its external debt service payments. The model predicts 96% of the total outcomes correctly and 80% of the debt payment suspension cases correctly.

After almost nine years, the Third World debt crisis has become accepted as a political crisis, not just an economic crisis. However, political variables that may help to explain why some countries have been able to continue to service their external debt and experience continued economic growth while many others have suspended their debt service payments are seldom specified: differentials in the government's political capacity (the ability to extract resources from society and redirect them for state purposes) and differentials in the government's management of macroeconomic policy are often summarized as à lack of political "will." Yet if political "will" helps to explain why some heavily indebted countries have continuously encountered severe debt service problems while others have not, there is need to specify its elements and identify the political mechanisms by which they affect the probability of a country's suspending payments on its debt service.

I use three variables representing different aspects of a government's political performance to evaluate the probability of a government's suspending payments on its external debt, taking into account differences in the level of debt service burden and the susceptibility of the debtor country to shocks from the international economy. The first is the government's political-capacity-as-fiscal-extractive-capability. The second variable is capital flight. It represents a loss of confidence, on the part of people who benefit from market-oriented policies, in the government's ability to manage the country's political and economic problems. The third variable is also judgmental. It is an evaluation by a country's foreign creditors of the government's ability to manage the economy. The more a government manipulates macroeconomic policy to accommodate the demands of political constituencies, the lower its credit-worthiness will be rated in international financial markets.

The premise of this study is that the political weakness of many Third World governments with respect to extraction of revenues and allocation of public resources led to the excessive politicization of macroeconomic policy. These factors not only contributed to increased foreign borrowing but also hampered many Third World governments' abilities to adjust to shocks from the international economy in the 1980s and to continue servicing their foreign debt. The study's data base consists of a cross-sectional pooled time series for fifty-eight countries over the period 1970–84. There are 682 cases of complete data on all the variables, 90 of which are classified as default years. *Default* is used in the broad sense to designate any unilateral suspension of payments by a government on its external debt, thereby failing to meet its original debt service obligations.

The Political Attraction of Foreign Borrowing

Political Reflections from a Fiscal Mechanism

The extractive capacity of the state is a partial reflection of the country's domestic politics. Who is taxed, how they are taxed (direct taxes, indirect taxes, taxes on trade, etc.), and how much they are taxed are political questions par excellence; hence the relevance of Gabriel Ardant's characterization of the fiscal system of the state as the "transformer" of the economic infrastructure into political infrastructure (Ardant 1975, 220). In this sense the fiscal system helps characterize the political issue areas in a society.

Not just the ability to tax but the ability to allocate or, more importantly, *reallocate* public revenue is related to the probability that a country will suspend payments on its external debt. Allocation involves political winners and losers. Reallocation often involves upsetting the political status quo. Therefore, a government that exhibits a low capacity to extract resources and a low capacity to allocate public resources for state purposes in the face of domestic political opposition and other constraints will have a strong incentive to resort to foreign borrowing and to printing more money (seigniorage). When both increased extraction and reallocation are foreclosed, foreign borrowing and seigniorage become very attractive.

The constraints on the ability to extract revenues or reallocate expenditures are intensely political. Improving collections of existing taxes, raising tax rates, broadening the tax base, or reallocating public expenditures will usually provoke political groups either within the government's coalition or outside it. One reason why so many Third World governments rely disproportionately on taxes on foreign trade and are reluctant to expand their tax bases is that trade taxes directly affect the fewest domestic groups. Since political leaders wish to avoid provoking the groups that are the foundations of their political support, they are very slow to expand their tax bases, particularly if their support coalitions are fragile and internally divided.

Government Political Capacity and Foreign Borrowing

The temptation of many Third World governments with low political capacity to resort to foreign borrowing has little to do with calculations concerning real interest rates but is attributable to political weakness. Much of the foreign borrowing was often done to postpone the painful political choices associated with balancing the simultaneous demands of political legitimation and economic development with a low capacity for extracting resources from society. The imperatives of political legitimation often meant financing higher levels of consumption (reflected in the share of government budget allocated to subsidies) than the country could afford. The imperatives of economic growth required that public expenditures be channeled into productive projects that would generate their own earnings. Promoting economic growth often calls for more market-oriented policies than the imperatives of distributive politics will allow.

The weaker a government politically, the more it resorted to foreign borrowing from commercial creditors to cover the gap between revenue and public expenditures. Where foreign capital was used to cover the public sector deficit, it had distributional effects similar to those of other categories of government spending. Thus, it is not surprising to find that the lower a debtor government's relative political capacity, the higher its budget deficit, the higher the budget shares spent on subsidies and government consumption, and the higher its level of foreign borrowing. I report elsewhere these findings (Snider 1990).

Nor should it be surprising to find that the lower a country's political capacity, the higher its external debt, rate of inflation, and debt service ratio. While most Third World countries lack the domestic capital markets to finance government debt, this is a weak explanation for the amount of borrowing witnessed in the 1970s. Certainly, one reason that many Third World governments resorted to foreign borrowing before 1980 was that real interest rates were close to zero and at times even negative. Under these conditions borrowing in foreign capital markets made sense even for governments that were not plagued with a low capacity to extract or to reallocate revenues.

However, when real interest rates began to increase to record levels in 1980, countries with average or higher political capacity were better able to adjust to this external shock by a combination of changes in fiscal policy and expansion of exports. Some of them could cut back on foreign borrowing or shift to official sources of credit. Hence, the average political capacity of countries that diversified their sources of external finance about evenly between commercial and official creditors might be noticeably higher than that of countries that relied increasingly on commercial creditors throughout the 1970s. By contrast, many of the countries that continued to borrow from commercial creditors even as real interest rates began to climb probably did so for political reasons. Official creditors such as government aid consortia, the Paris Club, or multilateral lending institutions like the World Bank or the International Monetary Fund (IMF) were inclined to insist on fiscal restraint and to limit levels of lending to amounts they considered a Third World borrower could use effectively. Accepting such restraints would have caused political problems at home, particularly if countries with low political capacity were borrowing abroad primarily to finance high levels of consumption. By contrast, the large money-center banks, concerned with remaining competitive in a profitable financial market, were less discriminating (Makin 1984, 147–52; Trebat 1983, 3). When the negative shocks after 1980 sent many Third World borrowers to their banks for short-run adjustment loans, a majority of these were countries with low political capacity. This may help to explain why the countries that the IMF designates as predominantly "market borrowers" for the years covered in this study suspended payments on their external debt service almost six times as often (forty-eight times) compared to the Third World countries the IMF classifies as "diversified borrowers." The latter group's external borrowings were divided fairly evenly between official and commercial creditors (IMF 1988: 106). The political capacity of the diversified borrowers in this study is significantly higher than that of predominantly market borrowers.

Political Capacity, External Borrowing, and World Commodity Prices

The interaction effects of low political capacity and a preference for financing from foreign commercial creditors can be compounded by the tendency of politically weak Third World governments to become dependent on commodity-based revenue when world commodity prices are relatively high. Commodity booms such as the one that occurred in the mid-1970s are a very tempting source of revenue to weak governments owing to the relative ease of taxing commodity-based exports as opposed to income. This becomes particularly attractive in cases (such as oil) where the government controls the commodity. This sort of secular rise in income, however, puts governments under pressure to make commitments that cannot be easily abandoned as commodity prices fall.

The income from commodity exports creates a double windfall if it increases the temptation to resort to more foreign borrowing, but as Haggard and Kaufman note, this windfall has three serious political consequences (1989, 217). First, it undermines any political impetus to undertake adjustments that have distributive consequences, so that politically painful decisions are deferred. Second, it can increase the range of political claims on state-controlled resources from rent- and revenue-seeking groups as well as from spending and planning constituencies within the government itself. This effect can be particularly serious in countries like Mexico, Argentina, or Egypt, where state-owned enterprises play an important role in the economy. Finally, these secular windfalls provide governments with resources than can be used for political ends whether through corruption, pork barrel projects that cement geographically defined bases of political support, or the expansion of subsidies and entitlements.

This may explain why increases in world prices for commodities such as agricultural raw materials (a principal category of developing country exports) are associated with a cycle of increased government borrowing, larger fiscal deficits, and ultimately a balance-of-payments crisis and the possible cessation of external debt service payments. If so, these debt service defaults may be more frequent when world prices for agricultural raw materials are rising than when they are falling.

Government Political Capacity and the Politics of Adjustment

When a country with previous access to foreign capital finds this access cut off or drastically reduced, the government has to mobilize domestic resources and foreign exchange to maintain its debt service payments. Either domestic savings, exports, and government revenue must increase or investment, imports, and government spending must decrease. Adjustment has to involve some combination of these. As it becomes harder to finance the public deficit, government spending is cut and employees, suppliers, and consumers of the public sector are directly affected.

If a developing country initially succumbed to the temptation of foreign borrowing out of political weakness, its ability to continue servicing its debt when the supply of foreign capital is reduced or terminated will be constricted for at least two reasons. First, the effects of the government's initial low extractive capacity are now compounded. A weak ability to raise and allocate revenue to cover the public spending-revenue gap without fear of adverse political repercussions was one reason why it resorted to foreign borrowing. When it is hit by exogenous economic shocks—higher real interest rates, a drop in world prices for its principal exports, a decline in the availability of foreign capital—it must not only meet the original spending-revenue gap from domestic resources, it must also meet its increased debt service obligations.

Second, the more it used the foreign borrowing to finance the public sector deficit, the more the distributional effects duplicated those of other forms of government spending. It built up constituencies who see such public spending as an entitlement. This is particularly so when the public borrowing was done by parastatal firms. This has often been the case in many of the heavily indebted countries. These organizations' employees benefit from increased salaries and wages that are often higher than wage and salary levels set competitively by market conditions. Vendors and suppliers to such enterprises benefit through increased orders, often at higher prices than could be obtained in a competitive market. Consumers of the products and services of state enterprises benefit by paying subsidized prices. If the public deficit can no longer be financed by foreign borrowing and the government curtails public spending, the employees, suppliers, and consumers of the public sector suffer immediately.

The groups that do business with state-owned enterprises are generally holders of fixed assets such as factories, farms, and construction equipment. They are fixed in that they are tied to a particular location and economic activity. Fixed asset sectors generally include industry and agriculture.

By contrast, holders of liquid assets such as bank deposits can easily turn them into ready money and transfer them from one application to another. Liquid assets usually include finance, real estate, services, and domestic and foreign trade. Over time, market-oriented policies will strengthen liquid asset holders more than fixed asset holders, while sectorial policies will have the opposite effect (Frieden 1988).

When a country that previously had access to foreign capital is cut off, the holders of liquid assets will respond differently to a government's efforts to adjust to this new situation than will holders of fixed assets. The two types of asset holders may also respond differently depending on whether they are inside or outside the government's coalition. Liquid asset holders are better positioned to shift their wealth in response to changes in government policy and so are less likely to react to government policy changes with political protests. When liquid asset holders face uncertain or unfavorable government policies, they can retreat to market alternatives such as capital flight. When fixed asset holders' interests are threatened by changes in government policy, market alternatives are generally closed to them. Thus, they will react with political protests and demands for more favorable government action.

Government Political Capacity and Capital Flight

At this point, the government's political capacity becomes critical. If a politically weak government's most powerful constituents are holders of fixed assets, it is likely to succumb to their demands for sectorial relief, usually in the form of subsidies. One result is an increase in capital flight as holders of liquid assets respond to unfavorable government policies and increased economic uncertainty. High levels of capital flight indicate that the government is not able, for political reasons, to rely on market mechanisms to adjust to external shocks. The political reasons consist of pressures exerted on the government for sectorial policies that favor fixed asset holders. Worse, the capital flight means a loss of domestic resources to government authorities who seek to exercise some control over how those private funds should be used. This is why the lower a country's political capacity relative to other governments with similar resource endowments and the higher its capital flight, the greater the probability that a debtor country will suspend its debt service payments and seek rescheduling.

If capital flight represents the reaction to government policies by the holders of liquid assets, the share of public expenditures allocated to subsidies represents government responses to holders of fixed assets. The lower a government's political capacity, the greater the share of government budget spent on subsidies. Two observable consequences are increases in the government's budget deficit and increases in capital flight.

When commercial creditors become reluctant to continue lending, governments with low political capacity will be prone to suspend the payments on their external debt and to seek rescheduling. Debtor governments with higher political capacity will be better able to adjust to external shocks by making structural changes to redirect resources, restricting imports, and expanding exports. Should they need to seek rescheduling, they are more likely to be successful in implementing the necessary policy changes called for by the IMF in the rescheduling agreement and in surviving domestic political repercussions than debtor governments with low political capacity. For example, Haggard (1985) notes that the ideological projects of political elites and the political orientation of debtor governments shape their approach to economic management and IMF-style reforms. Officials start by manipulating macroeconomic policy for political ends. This produces large fiscal deficits, rapid monetary growth, and an overvalued currency. Governments then postpone or resist stabilization policies in anticipation of hostile reactions from key constituents (Haggard 1985, 511–12).

The foregoing discussion summarizes why one would expect that the lower a government's political capacity and the higher its capital flight, the higher the probability that it will encounter problems servicing its external debt. These measures, combined with a measure of the extent to which a debtor country's macroeconomic policy is oriented toward orthodox, market-oriented objectives, a measure of the government's debt service burden, and variables representing changes in world commodity prices and international interest rates, should reveal, with significantly greater probability than would be expected by chance, whether a Third World country is likely to suspend payments on its external debt.

Measuring the Political Performance of Governments

The concept of political capacity used in this study was originally developed by Organski and Kugler (1978, 1980). It represents government political performance on two dimensions: (1) the ability to penetrate society to exercise effective authority over as many citizens or subjects as possible within the state's jurisdiction and (2) the ability to extract resources from society in support of national objectives. The key to understanding the political effectiveness of Third World governments hinges on whether elites have the tools to extract human and material resources from their societies and the ability to mobilize these resources into a national pool and use them to promote government objectives. The availability of resources enables a government to carry out tasks imposed on the political system by competing domestic political actors and by pressures from the external environment. This definition of political capacity is not related conceptually to different types of political systems. Highly capable political systems need not be democratic, stable, or highly representative, no matter how desirable those attributes may be.

The political factors that determine levels of political capacity cannot be specified directly. Indirect estimates can be made that capture the relative differences in extraction across countries with similar levels of economic productivity. As noted earlier, tax revenues are a direct measure of government extraction. Without revenue there can be no government operations or any stability. Thus, tax collections are pursued relentlessly by governments and resisted vigorously by their populations.

The separation of political capacity from economic capacity for developing countries is based on a fiscal problem common to all of them: their total revenues fall far short of desired government revenue. Fiscal economists have shown that in developing countries, governments extract right up to the limit of available resources (Bahl 1971; Chelliah 1971; Lotz and Morss 1967, 1970; Tait, Grätz, and Eichengreen 1979). Therefore, differences in resource (tax) base and fiscal structures (i.e., how different countries extract revenues from the same sector) account for almost all the economically produced differences in tax revenues.

The concept of political-capacity-as-fiscal-extraction can be represented as a ratio of actual tax revenues gathered by the government to the tax revenues predicted on the basis of the productivity of the population, natural resources, and reliance on the foreign trade sector. It is based on establishing two reference points:

$$\text{political capacity} = \frac{\text{actual extraction}}{\text{predicted extractive capacity}}$$

The actual revenue extracted is measured with respect to a country's resource base. This is a government's tax revenues as a percentage of its GDP. A much greater challenge is to obtain an accurate approximation of predicted revenue extraction, given the tremendous variations in populations, natural resource endowments, and levels of economic development among the developing nations used in this study.

Measuring Estimated Extractive Capacity

Three sets of controls are introduced to adjust for differences in economic conditions and resource endowments over time. First, the proportion of total income that can be extracted by the government depends in part on the general productivity of the population. The share of income that governments of relatively affluent industrial societies can collect can be substantial. In poor societies where subsistence farming remains widespread and industrialization limited, this share is low. Per capita income is useful for gauging the productivity in industrial societies, since it reflects differences in the resources that a government can tap. It is not an adequate measure for developing nations that still depend heavily on subsistence farming and in which market and nonmarket economies exist side by side. Thus, the real productivity of the population is higher than national accounts register, since the government generally does not have access to nonmarket resources. Therefore, the agricultural share of GDP is a more accurate control for variations in productivity when dealing with the extreme differences in potentially taxable resources available to the highly differentiated collection of countries that constitutes the developing world (Chelliah 1971, 282, 291–93; Chelliah, Bass, and Kelly 1975, 197–240). Nor is per capita income particularly suitable from a theoretical standpoint. A fundamental assumption underlying the measurement of political capacity is that economic weakness is not related to political weakness. If the two are related, it is only via some sort of structural attribute that may be present in some Third World economies.

Differentials in resource endowments are controlled by mineral production as a percentage of GDP. Large mineral deposits give a government a considerable advantage in collecting revenues, particularly if these mineral deposits include oil. The mining sector generally yields a larger surplus than other sectors. In addition governments can increase revenues by imposing taxes on mineral production on a few large firms without politically penetrating the general population. Absent this control, governments such as Ecuador, Mexico, or Nigeria would be assigned higher levels of political capacity than is actually warranted.

The third control concerns the revenues generated by foreign trade. The tax structures of politically weaker states are based far more heavily on trade taxes as a percentage of total revenue than domestic taxes on goods and services or direct taxes on income and profits. Governments, particularly weak ones, often seek to have their cake and eat it too by acquiring revenues while avoiding direct confrontations with the people who must pay the bill. Trade taxes are one way of raising revenue while minimizing direct contact with the national population. Trade taxes are relatively easy to collect because imports and exports are channeled through centralized port facilities. They are easy to extract precisely because a government does not have to be very effective in establishing its authority throughout the realm (political penetration) in order to collect them. The size of the foreign trade sector is measured by the value of exports as a percentage of GNP. The export ratio is more appropriate than using an import ratio or the sum of exports plus imports as a percentage of GNP when the foreign trade variable is intended to reflect the size of the base that is amenable to corporate income or export taxation. If it is more feasible, administratively and politically, to tax large exporters than it is to tax other domestic producers, the tax ratio will be higher where the export ratio is higher (Bahl 1971, 587).

An estimate of predicted revenue extraction is generated by an econometric model using multiple regression that incorporates the economic controls previously described plus controls for trend. The model parallels those used by political scientists previously and applied to other policy domains (Kugler and Domke 1986; Kugler, Organski, Johnson, and Cohen 1983; Organski and Kugler 1980). The principal difference is that the model used to estimate predicted extraction includes explicit controls for the extreme differences in taxable resources and tax structures found in high-income, oil-exporting countries. I have presented it in detail elsewhere (Snider 1988). Relative political capacity (RPC) is the ratio of observed tax revenues (less social security contributions) as a percentage of GDP to the predicted tax revenues adjusted for differentials in resource endowments and economic conditions.[1] When a government is performing close to the norm compared to other governments with similar resource bases, this ratio will be close to *one*. When it is extracting below this norm, the ratio will be considerably less than one. Governments with an extractive capacity that is considerably higher than what would be expected given their resource base will exhibit ratios significantly higher than one.

Relative political capacity is a better measure than tax-revenues-as-a-percentage-of-GDP because it measures estimated extractive capacity. A simple tax ratio provides an indication of how well a country is able to collect taxes in a given year compared to earlier or later years. Since countries have vastly different resource endowments, the tax ratio does not indicate how well one country is doing in relation to other countries with similar resource bases because there is no common reference point. The reference point is a country's estimated extractive capacity.

Capital Flight

Capital flight is another manifestation of the political weakness of governments. It represents a negative assessment on the part of politically and economically relevant groups of the government's effectiveness in managing the country's political and social problems. It is inversely related to measures of political strength like RPC. The motivation-based element in capital flight is one thing that makes it difficult to operationalize, since it is driven by motives and perceptions that are difficult to isolate and subject to change.

I use a fairly narrow measure of capital flight based on a procedure developed by Cuddington (1986). It is operationally defined as net short-term capital outflows of the nonbank private sector plus recorded errors and omissions. The underlying assumption is that capital flight is essentially a short-run, "hot money" phenomenon and that a large number of capital flight transactions are hidden from the authorities and will only appear residually as errors and omissions in balance-of-payments statistics. Such short-term funds respond quickly to changes in expected returns or in the risk attending political or financial crises; the tightening of capital controls or heavier taxes; and the fear of major devaluations of the domestic currency or actual (or anticipated) hyperinflation (Cuddington 1986, 2). This definition typically refers to capital export by the private nonbank sector. Although there are some variations in the calculation of capital flight based on information contained in the notes in the IMF's *Balance of Payments Yearbook*, the operational definition generally includes errors and omissions plus "short term, other sectors" or "short term, other sectors, other assets."

Country Credit Rating Index

A country's credit rating is a reflection of how well the government is perceived by its creditors to be managing its economy. A variable that functions as a broad evaluation of a government's performance in managing the economy is the *Institutional Investor's* country rating index. This is a judgmental index based on an annual survey of ninety bankers involved in international lending. While the methodologies used to construct the index are proprietary, it appears to take into account the politicization of the government's management of the economy to the extent that it deviates from market-oriented policies and thus affects the perceived risk of lending. The bankers are asked to assign scores on a scale of zero (least creditworthy–highest probability of default) to one hundred (no risk). The scores are averaged across banks using a weighting procedure that gives representation to the larger banks with the highest worldwide exposure and the most sophisticated country analysis capabilities (*Institutional Investor*, vol. 18, 1979, 243–46). The index, for example, is strongly and negatively associated with the rate of inflation. Economic factors alone can explain *how* inflation happens but not why. Whether to pursue inflationary policies and how much inflation is permissible are essentially political decisions. The index is negatively correlated with inflation and certain economic and structural characteristics and is positively but weakly related to a country's political capacity.[2]

Measuring Debt Burden and External Economic Shocks

The ratio of debt service payments to export earnings is a critical indicator of a country's debt service burden. A high debt service ratio indicates a heavy burden on a country's resources and is related to a higher risk of default. Debt service is a fixed obligation, so any shortfall in foreign exchange earnings must be offset by exchange reserves or import reduction considerations. The interest on foreign debt is the most rigid element of a country's balance of payments. Since it is contractually fixed and is a recurring charge on a country's economy regardless of the borrower's fortunes, it is an indicator of more than just short-term "disturbances." Data on debt service ratios were drawn mainly from the World Bank's *World Debt Tables* (various editions).

Recall commodity prices are believed to have a double-edged impact on a Third World government's debt conundrums. First, commodity price booms may have encouraged politically weak governments to resort to additional foreign borrowing. Second, the debt-servicing capacity of Third World borrowers depended heavily on export earnings from primary commodities. When commodity prices fell in 1980, these countries' abilities to service their foreign debts were severely degraded. While the impact varied across different categories of primary products, the countries experiencing some of the most serious debt-servicing problems were those suffering from declines in the world price of agricultural raw materials. Countries that exported mainly petroleum products or nonfuel minerals were not generally as vulnerable.

Countries were assigned one of four commodity price indexes according to the commodity grouping that accounted for 50% or more of their exports in 1980 based on data from the IMF (1988, 104–5): agricultural raw materials, minerals, fuels, or manufactures. The countries that are primarily exporters of manufactured goods were used as the comparison group. These are India, Israel, South Korea, and Yugoslavia. All commodity prices are indexes where 1980 = 100. They track the yearly rises and falls in commodity prices in relation to 1980, the year that marks the beginning of the world recession and the beginning of higher nominal and real interest rates. Fluctuations in the prices of agricultural commodities, minerals, fuels, or manufactures were created by taking the differences between the yearly index figure and the base year. Data on world commodity prices were drawn from the IMF (1987a, 1987b).

Apart from the drop in commodity prices, the most devastating external shock was the abrupt rise in real interest rates in 1981–82. Together these two variables produced a sea change in the debt servicing capacity of Third World countries (Cline 1984, 7–8, 11–14). As long as export earnings were growing at a higher rate than real interest rates (as occurred throughout the 1970s), few countries encountered serious debt-servicing problems. However, when real interest rates increased sharply in 1981, the effects of this shock were intensified by the accompanying drop in world commodity prices.

The floating interest rates on Third World commercial lending are a critical mechanism by which shocks in the international economy are transmitted to individual debtor countries. Interest rates on commercial sovereign loans are measured by the London Interbank Offer Rate (LIBOR) on U.S. dollar deposits, which ties it to dollar inflation. This is the rate of interbank transactions and reflects the banks' cost of raising funds. Since most longer-run increases in interest rates result from higher inflation, banks have protected themselves from inflation losses on their Third World lending. The actual interest charged for loans is this base rate, reflecting the banks' funding cost plus some percentage point reflecting anticipated economic conditions and the risk of lending to a particular borrower.

The rate I use is the six-month LIBOR. This reflects the banks' practice of extending maturities while minimizing their risks in Third World lending. The syndicated loans to developing countries were usually medium-term loans, which meant the banks were committing themselves to a credit over seven to ten years. However, to minimize interest rate risk, the banks set a new interest rate for the borrowing country on the basis of what it cost them to obtain funds. Data on the LIBOR are from the IMF's *International Financial Statistics.*

A distinction was made between the countries that are primarily market borrowers (whose external creditors are generally commercial lenders charging market rates of interest) and those that are mainly diversified or official borrowers. Countries that divided their sources of foreign borrowing about evenly between commercial and official creditors may have done so more in response to changing relative costs of borrowing from different types of creditors than for political reasons. The diversified borrowers' average political capacity, as measured in this study, is significantly higher than for the country population as a whole ($t = 3.280$; $p \geq |t| = .001$, $N = 682$). Official borrowers almost always receive credit at below-market interest rates. These countries' decisions to suspend payments on their external debts or the timing of any reschedulings could be determined by a different set of political or economic factors than those faced by predominantly market-borrowing countries. I designate countries market borrowers if they obtained two-thirds or more of their external borrowings from commercial creditors. This is the definitional guideline used by the IMF (1988).[3]

The Dependent Variable

The outcome to be predicted is a government's unilateral suspension of debt service payments without having made a formal request for rescheduling by allowing arrears to accumulate. The sample contains only directly observable information on the occurrence or nonoccurrence of such suspension. Therefore, the dependent variable is a binary variable. If a country suspended the payments on its external debt service a value of one is assigned to that country for that year. The value zero is assigned to all other years. The actual probability of a decision to suspend external debt service payments lies somewhere between one and zero.

The ordinal nature of the observed dependent variable is due to methodological limitations in data collection, which permit only the identification of extreme portions (one or zero) of an otherwise interval variable. Thus, logit analysis is used to analyze the data since it is a method specifically developed to deal with a binary-valued dependent variable.

Research Design

The data analysis uses a pooled time series data set consisting of fifty-eight countries for the years 1970–84 (N = 682 observations with complete data on all variables). A pooled time series design permits valid generalizations concerning a large, representative sample of countries (space) across time (several years for each country). The pooled time series structure, however, raises statistical problems arising from the possibility of autocorrelated error terms within each country and heteroscedastic disturbances (unequal variances) across a group of countries at any one point in time. Ordinary least squares regression is not always suited to a pooled time series data set, as it cannot simultaneously control for heteroscedastic disturbances in the cross-sectional analysis and control for autocorrelation in the error terms in the time series. Consequently, tests for serial correlation such as the Durbin-Watson *d*-statistic often yield unreliable results.

However, since the method of estimation is logit analysis using a dichotomous

Table 1. Logit Estimates of Debt Rescheduling Probabilities

Variable	Maximum Likelihood Coefficient	t-Score
Constant	-5.104	-5.9****
Relative political capacity	-1.533	-3.0****
Capital flight	.0004	2.3****
Debt service ratio	.082	4.3***
Institutional Investor country credit rating	-.199	-8.6****
Change in world price of agricultural commodities	.064	2.8***
Change in world price of minerals	-.231	-5.8****
Change in world price of oil	-14.95	-2.1*
Six-month London interbank offer rate, previous year	.114	2.0*
Market borrower	3.544	6.0****
Number of cases	682	
Chi-squared	327.56	
Probability > chi-squared	.0000	
Log-likelihood ratio	-102.27	
F or -2 × log-likelihood ratio	409.08	

$^{*}p \leq .05$
$^{**}p \leq .025$
$^{***}p \leq .005$
$^{****}p \leq .001$

dependent variable, a straightforward way to determine whether trend is a problem is to include it as a variable and assess its effects on the outcome. This was done by including year as a variable in a preliminary analysis. Since it had no decisive impact, it was removed from the model.

Data Analysis

The data analysis addresses the question whether measures of government political performance add significant unique information about which countries are most likely to experience serious debt problems when the size of their debt service burden and susceptibility to shocks from the global system are also taken into account. This question is tested in three logit analyses.

Logit analysis is analogous to multiple regression analysis when the dependent variable is dichotomous. The maximum likelihood coefficients serve the same purpose as regression coefficients in that they indicate the amount of difference in the dependent variable accounted for by each explanatory variable. The coefficients' standard errors and t-ratios have the same meaning as in regression. They indicate the relative explanatory power of each independent variable and the level of its statistical significance ($P \geq |t|$). In logit analysis the best measure for the power of the whole equation is the percentage of correct predictions. There is no generally accepted summary measure of proportionate reduction of error analogous to the R-squared in multiple regression. Instead, the logit model is evaluated in terms of its ability to correctly classify the cases according to their values on the dependent variable.

The model in Table 1 shows the impact of the political performance variables plus the debt service ratio and the external shock variables on the probability of debt rescheduling. The magnitude of the t-ratios indicates that all of the causal variables have an empirically strong—as well as a statistically significant—impact

Table 2. Logit Estimates of Debt Rescheduling Probabilities: Predicted and Actual Outcomes

Debt Payments Actually Postponed	Predicted Debt Payment Suspensions No ($\varrho \leq .5$)	Yes ($\varrho \geq .5$)	Total
No	583	9	592
Yes	18	72	90
Total	601	81	682

Note: Chi^2 = 459.73, $\varrho >$ chi^2 = .000 (degrees of freedom = 1). 655/682 = 96% total outcomes correctly predicted; 72/90 = 80% debt payment suspensions or reschedulings correctly predicted.

on predicting the probability of debt rescheduling. The signs of the coefficients are in the anticipated direction. The lower a debtor country's political capacity and its rating on management of the economy, the higher the probability of debt-servicing problems. The higher its debt service burden and capital flight, the higher the probability of debt-servicing problems. If it is predominantly a market borrower, the probability that it will default is higher.

The variables representing the shocks from the international system also behave as expected. The higher the previous six-month London Interbank Offer Rate, the higher the probability that a Third World borrower will encounter debt-servicing problems. The double-edged impact of the world price of the three main commodity export groups is also clearly evident. Of particular interest is the positive impact of agricultural raw material prices on the probability of suspending debt service payments. The boom in agricultural raw material prices may have prompted many governments to resort to additional foreign borrowing. However, when real interest rates rose, many agricultural raw material exporters could not earn enough to sustain their level of debt service even when agricultural raw material prices were higher than the base year. Countries that export mainly fuels or nonfuel minerals present a different picture. The sign of the latter two commodity groups is negative. This indicates that the prices of these commodities have to drop below the base year in order to increase the probability of debt payment suspension. Since fluctuations in world prices of manufactures showed no significant impact on the outcome variable, it was deleted from the model.

The cross-tabulations in Table 2 show the proportion of defaults that are correctly predicted by each equation. The model correctly predicts 96% of the total outcomes and 80% of the debt payment suspensions correctly. This is far better than would be expected by chance.

Sensitivity Testing

One way of determining whether the measures of political strength add unique information about which countries may suspend payments on their external debt is to run the model again without the political strength variables. This interpretation ignores the possibility that a government's political weakness is also indirectly mediated through the politicized management of the country's economy. When RPC is removed, the remaining variables behave about the same as they do in Table 1. This null model correctly predicts almost as many total outcomes (93%) as the original model. However, the number of cases of debt payment suspension correctly classified drops by

Table 3. Debt Payment Suspensions Predicted by the Model but Not Predicted by Null Models

		Probability of Debt Payment Suspension		
Country and Year	Relative Political Capacity (RPC)	RPC Model	RPC Removed	RPC and Capital Flight Removed
Brazil (1982)	.659	.927	.895	.423
Central African Republic (1980)	.639	.931	.308	.310
(1981)	.594	.935	.312	.332
(1982)	.663	.947	.441	.481
Costa Rica (1982)	.833	.896	.372	.390
Dominican Republic (1982)	.355	.915	.305	.331
(1983)	.395	.505	.488	.498
Guyana (1980)	1.292	.801	.307	.308
Honduras (1982)	.765	.814	.254	.295
(1983)	.714	.922	.360	.392
(1984)	.732	.968	.496	.512
Mexico (1982)	.567	.804	.748	.299
Niger (1983)	.594	.823	.127	.129
Nigeria (1983)	.789	.664	.604	.492
(1984)	.733	.605	.465	.487
Sudan (1979)	.170	.856	.090	.087
(1980)	.166	.964	.321	.304
(1979)	.170	.856	.090	.087
Togo (1979)	.937	.555	.073	.071
(1980)	1.338	.603	.162	.159
Yugoslavia (1983)	.416	.529	.357	.461

20%. Thus, the inclusion of a specific measure of a government's political strength does add significant unique information about which countries are likely to suspend payment on their external debt when their debt burden and susceptibility to shocks from the international system have also been taken into account. When capital flight is also removed the total number of outcomes correctly predicted declines by only four percent from the original model, and the number of debt suspensions correctly predicted drops another two percent.

Incorrect Predictions

Table 3 summarizes RPC scores and probabilities of default for the twenty debt suspension cases correctly classified by the model but not predicted when RPC and capital flight were removed. Two cases, Dominican Republic in 1983 and Honduras in 1984, are so close to being correctly classified that the difference between correct and incorrect classification could be attributed to measurement error.

Looking at these twenty cases as a group, their average *Institutional Investor* credit rating is significantly higher (t = 3.063 $p \geq |t|$ = .002) for the debt suspension cases it failed to predict correctly than for the default cases that the null models classified correctly. The average debt service ratio was no different from the average for all 682 cases, whereas it was significantly higher for the countries the null models correctly classified. By contrast, the average RPC score for this group of cases is .636, significantly lower than the mean RPC score for all cases (.967) or for all ninety debt suspension cases (.883). It is also significantly lower (t = 2.355, $p \geq |t|$ = .019) than

Table 4. Debt Suspension Cases That Were Not Predicted

Country and Year	Relative Political Capacity (RPC)	Probability of Debt Payment Suspension: RPC Model	RPC Removed	RPC and Capital Flight Removed
Chile (1972)	.908	.255	.150	.155
(1981)	1.267	.113	.174	.221
Gabon (1978)	.908	.179	.104	.122
Ghana (1974)	1.015	.049	.079	.070
Guyana (1979)	1.007	.165	.293	.301
Jamaica (1978)	1.566	.009	.020	.016
(1979)	1.372	.016	.069	.063
Malawi (1980)	.742	.072	.126	.127
(1981)	.721	.058	.109	.118
Pakistan (1972)	.776	.041	.040	.039
(1973)	.654	.227	.215	.205
(1980)	.526	.185	.231	.232
Peru (1978)	.721	.012	.133	.142
(1981)	.744	.014	.023	.026
Uruguay (1983)	.775	.350	.332	.403
Venezuela (1983)	1.047	.088	.152	.209
(1984)	1.093	.037	.058	.108
Zaïre (1976)	.895	.120	.143	.116

the average RPC score for the debt suspension cases the null models correctly predicted. The average level of capital flight for these cases is higher than the average for all ninety debt suspension cases (989.5 versus 726.9). The default cases that were not predicted when RPC was deleted but capital flight retained showed significantly lower average capital flight (108.8) compared to the debt suspension cases correctly classified by this model (1,138.9, $t = -2.401$, $p \geq |t| = .017$). When capital flight was removed, the second null model failed to classify the two debt payment suspension cases that marked the beginning of the debt crisis in 1982 (Brazil and Mexico) correctly. These differences suggest that low RPC and high capital flight may be able to effectively identify Third World countries that are encountering serious debt service problems when purely economic indicators reflect performance that is close to average.

Eighteen debt suspension cases were not predicted by the model. Their RPC scores and probabilities of debt payment suspension are presented in Table 4. The average RPC level of this group is .930, very close to the grand mean (.967) for all countries and significantly higher than the mean RPC level of debt payment suspension cases correctly classified (.871).[4] The average level of capital flight (47.87) is significantly lower than the average for all ninety debt suspension cases (726.89) and those correctly predicted by the model (896.65).

The Uneven Effects of External Shocks

Another important difference concerns the average level of interest rates. The average LIBOR rate immediately prior to the suspension of debt service payments for the ninety cases where this occurred was 12.8%. It is 13.5% for the seventy-two cases correctly predicted by the model, much lower than the average

LIBOR for the eighteen debt payment suspensions that the model did not predict (9.8%). This is almost identical to the average LIBOR rate (9.5%) for the countries that did not suspend payments on their debt service. The average LIBOR rate was also significantly lower (11.6%) for the twenty default cases not predicted by the two null models ($t = -3.429$, $p \geq |t| = .001$) than for the cases they classified correctly. These differences suggest that in cases where governments' RPC was close to one standard deviation below the norm (between .62 and .66), interest rates do not have to increase nearly as much for a government to suspend payments on its debt service as in cases where RPC was closer to average. Otherwise, the causes for default probably lie outside the causal variables specified in the model.

Conclusions

The results of the logit analysis support the proposition that the frequency with which many lesser developed country debtors suspended payments on their external debt involves the government's political performance and the politicization of economic performance, not just economic performance or shocks from the international economy. When the direct effects of political capacity are removed, the total number of outcomes correctly predicted drops by only 3%–4%, but the number of cases of debt payment suspension correctly predicted drops by 20%. Thus, the inclusion of explicit measures of a government's political strength adds unique explanatory and predictive information concerning which developing countries are most likely to encounter serious debt servicing problems when the size of their debt burden and their susceptibility to external shocks are also taken into account.

Much of the literature on the debt crisis stresses the debtor countries' policy "mistakes" with respect to their patterns of external borrowing or in the adjustment policies undertaken in recent years. The results of this study suggest that many of these policies were not mistakes in the sense that the debtor governments misunderstood their implications or did not foresee the outcomes of their actions. Such "mistakes" are often symptoms of deeper political and social problems that have plagued many developing countries for over three decades. Most finance ministers and central bankers know what measures must be taken to put their economic houses in order, but they also know these measures do not have enough political support to be enforced effectively (Kuczynski 1988, 123). The economies of many of these countries exhibit severe inequalities in income, which in turn provoke intense political conflicts. In some cases (e.g., Argentina under Raúl Alfonsin, the Philippines under Ferdinand Marcos), the governments may have been too weak to resist the demands for spending from highly mobilized social groups. Most of these governments could not raise taxes on the wealthier strata of society or chose not to. Thus, on both the spending and the revenue side, political institutions repeatedly failed to keep the demands for government spending in line with the government's limited extractive capabilities. Foreign borrowing in the 1970s and early 1980s offered a short-term way out of these political dilemmas by allowing governments to finance large budget deficits without incurring high inflationary costs in the short term that would have been the case had they borrowed from their central banks.

The most important implication of these findings is that the economic and fiscal reforms prescribed by the IMF, the commercial banks, or the creditor governments will probably produce little real improvement in the debtor countries' economic performance unless they are implemented in tandem with political reforms.

The latter are much harder to initiate and sustain than the former. What is politically threatening to debtor governments is the question of who will bear what share of the burden of putting the country's economic house in order. The requirements of economic stabilization and structural adjustment inevitably shift benefits and resources from some economic sectors and social groups to others. Thus, economic adjustment measures inherently involve the politics of winners and losers, since they involve drastic changes in public spending allocations. Many of the prospective losers are the very personnel on whom the task of implementing economic reforms may fall. The lower a debtor government's political capacity, the more its stabilization policies will be constrained by the condition that the existing winners and losers represent the only political equilibrium that is possible. Hence, it will tend to compensate key constituencies (including the bureaucracy) that lose from stabilization or adjustment programs in ways that undermine the original purpose of those measures.

Notes

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1. Social security taxes are subtracted from total tax revenue because they are basically nondiscretionary. They can only be used for specific purposes defined by law. Hence, the government has no real latitude in determining how such revenues are spent. In addition, not all developing countries collect social security taxes. If social security taxes are not deducted from total tax revenues, the countries that collect them would appear to be extracting much larger shares of resources from their societies in comparison to other developing countries (with similar economic structures and resource endowments) than they really are.

2. The correlation between the index and inflation rate is −.25 (N = 682). Its correlation with RPC is .011 (N = 682). The index is also correlated with a country's main export category (primary commodities or manufactures), level of industrialization, and severity of currency controls. Since the *Institutional Investor* does not furnish complete data for all the countries and all the years included in the data analysis, missing data were replaced with estimated values generated by analysis of covariance. In addition to inflation, the following variables were used to generate predicted values: (1) level of industrialization (primary producer, semiindustrialized, newly industrialized); (2) rate of growth in GDP; (3) type of borrower (market, official, or diversified, as defined in IMF 1988); (4) severity of currency controls (free, liberal, strict, dictatorial—classifications and definitions from Cowitt 1986, 21); and (5) trade policy orientation (strongly outward-oriented, moderately outward-oriented, moderately inward-oriented, and strongly inward-oriented—as classified in Greenaway and Nam 1988, 423–25). The adjusted R-squared = .83.

3. The countries I include are grouped by predominant creditor. Predominantly market borrowers are Algeria, Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Gabon, Greece, Guyana, Indonesia, Ivory Coast, Malaysia, Mexico, Nicaragua, Nigeria, Panama, Paraguay, Peru, Philippines, Portugal, South Africa, Uruguay, Venezuela, and Yugoslavia. Predominantly official borrowers are the Central African Republic, Dominican Republic, Ethiopia, Gambia, Ghana, Honduras, Jordan, Liberia, Malawi, Pakistan, Somalia, Sudan, Syria, Tanzania, Togo, Turkey, Zaïre, and Zamibia. Predominantly diversified borrowers are Burma, Cameroon, Costa Rica, Egypt, El Salvador, Guatemala, India, Israel, Jamaica, Kenya, Morocco, Niger, South Korea, Thailand, and Tunisia.

4. $t = 2.268$, $p > |t| = .024$, N = 682. This is based on an analysis of variance where RPC was the response variable and the number of cases in each prediction category was the experimental variable: When the model correctly predicted the occurrence of default, $y = 1$; $\hat{y} \geqslant .5$. When the model predicted default but none occurred, $y = 0$; $\hat{y} \geqslant .5$. When the default was not predicted but did occur, $y = 1$; $\hat{y} \leq .5$. And, when the model correctly predicted the absence of default, $y = 0$; $\hat{y} \leq .5$. This same criterion variable was used to check for significant differences among the means for capital flight, the debt service ratio, and country credit rating.

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RESEARCH NOTE

DEFENSE EXPENDITURES, ECONOMIC GROWTH, AND THE "PEACE DIVIDEND"

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Recent developments in Eastern Europe have created expectations of a "peace dividend" associated with reduced levels of U.S. defense expenditures. We present and empirically estimate a two-equation model for assessing the direct and indirect, immediate and delayed effects of changes in defense spending on economic growth in the United States.

The dramatic developments in Eastern Europe and the downgrading of the Soviet bloc's threat to Western security have created expectations of a "peace dividend" associated with massive cuts in U.S. defense spending. Secretary of Defense Dick Cheney has recently indicated his willingness to cut military expenditures drastically, while the chairman of the Joint Chiefs of Staff, General Colin Powell, proposed deep cuts in U.S. troops in Europe (*Newsweek*, 4 December 1989, p. 44). While it is still unclear whether a "dividend" will indeed be paid, most people believe that drastic changes in military spending have a direct and immediate impact on the economy. Our purpose here is to determine whether the impact of reduced levels of U.S. military spending on economic growth is (1) direct or indirect and (2) immediate or delayed.

Based on the literature on defense spending and economic performance in industrialized countries (e.g., Chan 1985, 1987; Denoon 1986; Lindgren 1984; Rasler and Thompson 1988; Smith 1980; Smith and Georgiou 1983), we hypothesize that reduced levels of military expenditures either promote growth directly or encourage investment, which in turn promotes growth. We shall summarize the theoretical basis for this argument and then offer an empirical analysis of the guns–growth thesis. We begin by explaining the links between defense spending and investment and between investment and growth.

Defense Spending and Investment

On the basis of the examination of more than a dozen studies (including Smith 1977, 1980; and Smith and Georgiou 1983), Lindgren concludes that there is a negative relationship between military expenditures and investment (1984, 376). Others similarly find that higher military spending has stifled investment while lower levels of military spending have encouraged investment (DeGrasse, McGuiness, and Ragen 1983, 73; Cappelen, Gleditsch, and Bjerkholt 1984, 368). G. Kennedy therefore concludes that the guns–investment trade-off does exist (1983, 198).

Certain scholars provide an explanation for this relationship (Smith 1980, 22–23; Huisken 1982, 6; Rasler and Thompson 1988). Private and (nonmilitary) public consumption account for more than half of the total output of the economy and are highly resistant to reductions. Accordingly, military spending and investment compete for the nonconsumption portion of the total productive capacity of the economy. Increased levels of military spending crowd out investment, while reduced levels promote investment. Moreover, nonpersonnel military expenditures and investment demands are directed at roughly the same industries (Huisken 1982, 6). Increased military demand may therefore cause supply bottlenecks that squeeze investment. Increased levels of defense spending may also entail higher taxes or government borrowing in the capital markets, funds that might have otherwise gone to investment (Chan 1985, 416), while reduced levels of military spending may lead to lower taxes or less government borrowing. Smith and Georgiou (1983) and Chan (1985) recommends the use of long time lags in empirical tests of the warfare–investment trade-off.

Investment and Growth

Investment is a crucial element of economic growth (Solow 1988; Ram 1986). According to one estimate, investment accounted for nearly one-fifth of the average 2.90-percent annual growth rate of the U.S. economy over the 1929–1982 time period (Denison 1985, 30).

Since military spending is hypothesized to have a negative impact on investment, it is expected that it will also have a negative, indirect impact on growth "by impeding the renewal and expansion of existing capital stock as well as the rate at which technical progress and innovation are spread throughout the economy" (Mosley 1985, 65). Conversely, cuts in military spending may stimulate investment, which in turn may have a positive impact on growth. P. Kennedy (1987) points out that economic growth is suffering in the United States because of imperial overstretch and increased military spending. Nardinelli and Ackerman (1976) find the effect of defense spending on net real GNP to be negative. However, as the Chan (1985) and Lindgren (1984) surveys of the empirical work on defense spending and economic growth show, the evidence for such a link is mixed: while some scholars find military expenditures to have a dampening impact on growth, others are unable to find any evidence for the guns–growth trade-off, while still others find a positive correlation—for example, from technological innovations and spin-offs (see Lindgren 1984, 380). A more recent study (Rasler and Thompson 1988) finds the trade-off to be particularly severe in the post–World War II United States. Chan therefore concludes that "the available evidence is not entirely consistent but generally it does not support the view that military spending promotes economic growth" (1987, 39). Following Smith (1980), DeGrasse, McGuiness, and Ragen (1983), Huisken (1982), and Mosley (1985), we hypothesize that to the extent that reduced levels of military spending encourage investment, they indirectly promote economic growth.

Research Strategy

To test the direct and indirect, immediate and delayed effects of military spending on economic growth we developed and empirically estimated a two-equation "indirect effects" model, representing investment and economic growth, respectively. The investment equation is based on the work of P. Clark (1979). The growth equation relies on the work of Solow (1988), Denison (1967, 1985) and Ram (1986).

The "Indirect Effects" Model

Investment. Our investment equation is a modified version of a model originally proposed by J. Clark (1917) and later modified by other economists (Chenery 1952; P. Clark 1979; and Koyck 1954). According to P. Clark (1979), this model has a superior statistical performance over other specifications and is practically estimable. Furthermore, it allows us to isolate the putative effect of military spending.

P. Clark (1979) compares five different models of investment: the generalized accelerator model, the so-called accelerator–cash flow model (see Eisner 1978), the neoclassical model (see Jorgenson 1967), the so-called modified neoclassical model based on the work of Charles Bischoff (1971), and a securities-value (Q) model (see Tobin 1969). P. Clark finds that while the neoclassical investment function, which explicitly takes into account capital cost, is theoretically appealing, it is nevertheless plagued by measurement problems. In contrast, the flexible accelerator model, which relates investment to changes (i.e., accelerations) in output over a number of time periods, performs best for both inside sample prediction and outside sample forecast (1979, 85–92).

The flexible accelerator model represents gross investment (*I*) as a distributed lag on national product (*Y*) plus depreciation, which is assumed to be a constant proportion of the capital stock (*K*) of the previous period:

$$I = \sum_{S=0}^{\infty} \beta_S \Delta Y_{-S} + dK_{-1}. \qquad (1)$$

The gross national product (GNP) is conventionally defined as the sum of consumption (*C*); investment (*I*); government purchases of goods and services (*G*); and net export, which equals total export (*EX*) minus import (*IM*). By disaggregating *G* into military expenditures (*M*) and nonmilitary expenditures (*NM*) and rearranging the terms, *Y* becomes a function of three components: production in the private sector (*P*), nonmilitary government expenditures (*NM*), and military expenditures (*M*).

$$\begin{aligned} Y &= C + I + G + (EX - IM) \\ &= [C + I + (EX - IM)] + NM \\ &\qquad + M \\ &= P + NM + M \end{aligned}$$

By the distributive law, the first difference of *Y* is equal to the sum of the first differences of its three components, that is, $\Delta Y = \Delta P + \Delta NM + \Delta M$. Substituting it into equation 1 results in:

$$I = \sum_{S=0}^{\infty} \beta_S \, \Delta P_{-S} + \sum_{S=0}^{\infty} \beta_S \, \Delta NM_{-S} + \sum_{S=0}^{\infty} \beta_S \, \Delta M_{-S} + dK_{-1}. \qquad (2)$$

To transform equation 2 into a statistical equation for estimation, we first allowed for only a finite number of lag coefficients for each component. We then divided both sides by *Y* to obtain the proportion of GNP invested by the business sector (our dependent variable). This is not only more interesting theoretically, but also reduces the potential heteroscedasticity problem in this equation, since the error variance of the level of investment tends to rise in proportion to the size of the economy. Finally, we added to the right-hand side of the equation a constant term (*a*), and an error term (*e*). The resulting equation is

$$\frac{I}{Y} = a_1 + \sum_{S=0}^{n_1} b_{11,S} \frac{\Delta P_{-S}}{Y} + \sum_{S=0}^{n_2} b_{12,S} \frac{\Delta NM_{-S}}{Y}$$

$$- \sum_{S=0}^{n_3} b_{13,S} \frac{\Delta M_{-S}}{Y}$$

$$+ b_{14} \frac{K_{-1}}{Y} + e_1 \qquad (3)$$

where I = gross private domestic investment in 1982 prices; Y = GNP in 1982 prices; $\Delta P = P - P_{-1}$, where P is private business output in 1982 prices; $\Delta NM = NM - NM_{-1}$, where NM is nonmilitary expenditures in 1982 prices; $\Delta M = M - M_{-1}$, where M is military expenditures in 1982 prices; and K = net value of capital stock in 1982 prices.

Growth. The second equation links gross investment, military spending, and other relevant variables to economic growth, which is measured by the growth rate of GNP in constant dollars. Here we extend the work done by Solow (1988), Denison (1967, 1985) and Ram (1986).

The simplest version of Denison's sources-of-growth model (1967, 1985) attributes changes in *levels* of gross output to changes in inputs of capital ($\Delta K = K - K_{-1} = I$) and changes in labor ($\Delta L$). Ram (1986) extends this model by explicitly distinguishing the government sector from the rest of the economy and therefore adds changes in government output (ΔG) as a third term in the equation. Dividing both sides of the equation by GNP of the last period transforms the left-hand-side variable from changes in output levels into economic growth rate. Ram further assumes that a linear relationship exists between the real marginal productivity of labor in a given sector and the average output per labor in the economy (1986, 193). Ram's growth equation then becomes

$$\frac{\Delta Y}{Y_{-1}} = \alpha \frac{I}{Y_{-1}} + \pi \frac{\Delta L}{L_{-1}}$$

$$+ \theta \frac{\Delta G}{Y_{-1}} \qquad (4)$$

In order to isolate the effect of military expenditures on economic growth, we then modified Ram's equation by disaggregating government output, measured by total government purchases of goods and services, into nonmilitary and military components ($G = NM + M$). By substituting this identity into equation 4 and further hypothesizing that increased levels of military spending tend to dampen economic growth while reduced levels promote growth, the resulting statistical equation is

$$\frac{\Delta Y}{Y_{-1}} = a_2 + b_{21} \frac{I}{Y_{-1}} + b_{22} \frac{\Delta L}{L_{-1}}$$

$$+ b_{23} \frac{\Delta NM}{Y_{-1}} - b_{24} \frac{\Delta M}{Y_{-1}} + e_2 \qquad (5)$$

where $\Delta Y/Y_{-1}$ = economic growth rate, where Y equals GNP in 1982 prices, and $\Delta L/L_{-1}$ = growth rate of employed civilian labor force.

Data

Data on GNP, the GNP implicit price deflator (1982 = 100), military expenditures, federal government purchases of goods and services, gross private domestic investment, export, import, and consumption, were taken from the Bureau of Economic Analysis' 1986 *National Income and Product Account of the United States, 1929–1982 (NIPA)*, supplemented by 1983–1987 data taken from the bureau's *Survey of Current Business* (July 1987 and July 1988). Capital stock figures were obtained from the same bureau's 1987 *Fixed Reproducible Tangible Wealth in the United States, 1929–1982*, supplemented by 1984–1987 data taken from the *Survey of Current Business* (August 1988). Data on employed civilian labor force were taken from the Bureau of Labor Statistics' *Employment and Earnings* (December 1983 and May 1989).

All data refer to the 1953–1987 time period (1953 is the first year for which data on all variables is available, and 1987 is the last). It should be pointed out that 1948–1952) data for three variables with long lags in equation 3 (i.e., military expenditures/GNP, nonilitary expenditures/GNP and private production/GNP) were also collected in order to ensure that degrees of freedom are not sacrificed and that all equations refer to the same time period. (See our discussion of the criterion for selecting the number of lags in this equation.) All data are in calendar years and in constant 1982 prices. Data in *NIPA* are reported in calendar years. The *Survey of Current Business* supplements data for these variables on a calendar year basis. Furthermore, the *NIPA* data set contains data in constant prices on most of these variables. However, *NIPA*'s data on military spending is in current prices only. The series was converted to constant 1982 dollars using the implicit price deflator for federal government purchases of goods and services, which is also reported in *NIPA*.

Data represent final outlays. While many studies of the determinants of defense budgeting in the United States have often used national defense data on different stages of the budgetary process (i.e., the services' requests, the president's budget, Congress appropriations, and Department of Defense expenditures), studies of the guns-versus-growth and guns-versus-butter trade-offs have virtually always used actual expenditures (see Mintz 1989). As Domke, Eichenberg, and Kelleher explain, "It is best to investigate final outlays—the bottom line of the ledger comprising the many alterations of the budget and its final implementation" (1983, 21). Since money authorized or awarded to national security is usually spent over a number of years, one cannot assess its impact on the economy accurately unless one examines actual expenditures. Several scholars (e.g., Griffin, Devine, and Wallace 1983) defend the use of the rate of change in spending as a proportion of the GNP. To avoid the problem of heteroscedasticity discussed earlier we measured the variables, except civilian labor force, as a proportion of GNP.

Methodology

Before turning to the results of our empirical analysis, several methodological issues should be addressed.

Estimation. The model can be estimated using single-equation estimator or by joint estimation of the entire system. The latter approach, often referred to as Zellner's (1962) "seemingly unrelated regression" (SUR) estimator, is asymptotically more efficient than the former, if the error terms of the equations are contemporaneously correlated. Judge and his colleagues point out, however, that in finite samples, greater efficiency of joint estimation can be obtained only when the contemporaneous correlation between disturbances in different equations is significantly different from zero (1985, 470; 1988, 452). Otherwise, estimating individual equations separately may be more efficient than joint estimation (see Kariya 1981, 382). Given several methodological problems detected in our model (lag structure in equation 3 and collinearity in equation 5), we decided to overcome these problems in individual equations first and only then to test whether the contemporaneous correlation of disturbances across equations was sufficiently significant to require joint estimation. Following the discussion of lag structures, collinearity, and autocorrelation we report the results of the Lagrange multiplier test (suggested by Breusch and Pagan 1980, 247).[1]

Lag Structure. Since our specification of the investment equation contains distributed lags yet there is no a priori information on the lag structure, an objective criterion is needed to determine the *number* of lags of *P, NM,* and *M* in equation 3. We relied on a standard information criterion suggested by Schwarz (1978) and recommended by Geweke and Meese (1981), often referred to as *SC*, to select the lag or lags to be included in the analysis beyond the current and the first lag of these variables. This criterion involves a statistic that incorporates a measure of the precision of the estimate and a measure of the parsimony of the statistical model. When a regression equation involving *q* parameters is fitted to data,

$$SC(q) = T \bullet \ln(\hat{\sigma}^2) + (\ln T) \bullet q, \qquad (6)$$

where T is the sample size and $\hat{\sigma}^2$ is the maximum likelihood estimate of the residual variance. The lag structure that numerically minimizes equation 6 is chosen in our case. It represents the optimal balance between precision and parsimony (Schwarz 1978).

Before calculating the *SC*s of different lag structures, we set the minimum number of lags as one and the maximum as five. In other words, the current and first lag of the three variables mentioned, plus the capital stock variable, were always kept in the equation in order to ensure the detection of their short-run impact on investment. P. Clark suggests that the accelerator effect might last up to five years (1979, 89). We therefore set the maximum number of lags to be five. (A larger number of lags would have significantly reduced the degrees of freedom in the model anyway—an increase in the maximum number of lags by one implies, for the three variables, a decrease of degrees of freedom by three.) The lag structure selected by *SC* is reported in the first column of Table 1. It should be pointed out that another popular information criterion, called (*AIC*), proposed by Akaike (1974), produced an identical lag structure.

Collinearity. Two right-hand-side variables (I/Y_{-1} and $\Delta L/L_{-1}$) of the growth equation are highly correlated, with a Pearson *r* equal to .90. While this may not be surprising inasmuch as the two inputs of production (capital and labor) are often used in an approximately fixed proportion (Judge et al. 1988, 862), collinearity makes it difficult to identify the separate effects of investment and labor on economic growth. This is manifested by the existence of large sample variances of the ordinary least squares (OLS) estimates of b_{21} and b_{22}. Indeed, the variance inflation factor of these coefficients is 5.5 and 5.7 respectively, an indication of severe collinearity. Although the best solution to the collinearity problem might be to introduce additional sample, this option is not feasible in this study. Consequently, we used the ridge regression estimator shown by Horel, Kennard, and Baldwin to be superior to OLS (1975, 107). We obtained a bias scale of $\hat{k}$ = .00023 and added it to the diagonal elements of the error covariance matrix of the growth equation. The results of the ridge regression estimates are reported in Table 1.

Autocorrelation. The Durbin-Watson statistic of the OLS residuals of equation 3 equals 1.12, which falls in the inconclusive region but closer to its lower bound than to its upper bound. To correct the serial correlation of the error term problem, we used generalized least squares (GLS) estimates of parameters in the investment equation.[2] Durbin-Watson statistics for equation 5 (1.91 for OLS and 1.68 for the ridge regression) indicates that no first-order autocorrelation exists in this equation.

Given the lag structure in equation 3, we assume that the disturbances of our model follow a first-order autoregression, that is, AR(1) (see Judge et al. 1985, 484):

$$e_t = Re_{t-1} + v_t,$$

where R is a two-by-two diagonal matrix with diagonal elements ϱ_1 and ϱ_2, and v_t is a vector of white noises. Since only ϱ_1 is found to be significantly different from zero, the contemporaneous disturbance covariance matrix becomes

$$E[e_t e'_t] = \begin{bmatrix} \dfrac{\sigma_{11}}{1 - \varrho_1^2} & \sigma_{12} \\ \sigma_{21} & \sigma_{22} \end{bmatrix}.$$

Next, we had to decide whether the model should be estimated jointly or separately. The decision lies in the contemporaneous covariance of disturbances *across* equations, i.e., σ_{12}. We therefore tested the hypothesis that $H_0 : \sigma_{12} = 0$. The Lagrange multiplier statistic, $\lambda_{LM} = 1.534$, is statistically *in*significant, for a chi-square distribution with one degree of freedom, at either the .05 or .10 level. By accepting the null hypothesis, we decided to use single-equation estimation.

Results

The results of the empirical analysis are presented in Table 1. An indirect effect of military expenditures on growth exists if the military expenditures variable in equation 3 has a statistically significant negative impact on investment, and investment has a positive and statistically significant impact on growth in equation 5. A direct effect exists if the impact of military expenditures on growth in equation 5 is negative and significant.

Turning to the investment equation, the current value of military spending and its first lag are both statistically nonsignificant.[3] However, the fifth lag of military spending as chosen by both Schwarz's and Akaike's information criteria is, as predicted, negative and statistically significant, lending support to the thesis that the impact of increased levels of military spending is to significantly decrease investment in the long run. According to Chan (1987), this is due to the tendency of policy makers to finance defense by running large budget deficits while postponing costs to future generations. These findings are robust in spite of our use of different measures of investment (i.e., fixed investment and nonresidential investment), different measures of defense spending (e.g., the interaction of military spending and defense buildups), and the use of different criteria for selecting the number of lags in the equation (i.e., *AIC* and *SC*).

Turning to the growth equation, the ridge regression estimates show that the impact of investment on economic growth is, as predicted, positive and significant (albeit only at the .10 level) and that the impact of military spending on growth is statistically insignificant, indicating that defense expenditures do not affect economic growth directly. The negative effect of military spending on economic performance is therefore mainly a result of its crowding-out effect on investment on the one hand and its inability to contribute positively to economic growth on the other. These results indicate the existence of an *indirect* effect of military expenditures (via investment) on growth and the lack of a *direct* short-term negative effect of military expenditures on growth. More importantly, they show that the impact is not immediate: only in the long run does military spending crowd out investment, which in turn reduces growth. When the interactive term of military spending and military buildup is introduced into the investment equation to account for the (final stage) of the Korean War buildup, the Vietnam War, and the Reagan defense buildup, the results are very similar,[4] indicating again the lack of a significant impact in the short run and the existence of a significant effect over the long run (on the importance of dividing the time series into subperiods of war and peace, see Hollenhorst

Table 1. The Impact of Military Spending on Investment and Growth

Variables	Regression Coefficient	Standard Error	Significance
Investment Equation			
Constant	.163	.042	$p < .01$
$\frac{\Delta P}{Y}$	.499	.053	$p < .01$
$\frac{\Delta P_{-1}}{Y}$	.410	.045	$p < .01$
$\frac{\Delta P_{-2}}{Y}$	.193	.043	$p < .01$
$\frac{\Delta P_{-3}}{Y}$	.188	.041	$p < .01$
$\frac{\Delta NM}{Y}$	-.679	.269	$p < .01$
$\frac{\Delta NM_{-1}}{Y}$	-.628	.244	$p < .01$
$\frac{\Delta NM_{-2}}{Y}$	-1.165	.295	$p < .01$
$\frac{\Delta NM_{-4}}{Y}$	-.516	.269	$p < .05$
$\frac{\Delta M}{Y}$	.133	.152	$p < .20$
$\frac{\Delta M_{-1}}{Y}$	-.074	.118	$p < .25$
$\frac{\Delta M_{-5}}{Y}$	-.262	.087	$p < .01$
$\frac{K_{-1}}{Y}$	-.008	.022	$p < .40$
Adjusted $R^2 = .89$, D-W = 1.81.			
Growth Equation			
Constant	-.067	*	*
$\frac{I}{Y_{-1}}$	.448	.305	$p < .10$
$\frac{\Delta L}{L_{-1}}$	.820	.314	$p < .01$
$\frac{\Delta NM}{Y_{-1}}$	1.133	.500	$p < .05$
$\frac{\Delta M}{Y_{-1}}$	.152	.303	$p < .40$
Adjusted $R^2 = .66$, D-W = 1.68.			

Note: Refer to p. 1286 for an explanation of the variables found in the table above.

*Inapplicable due to data transformation in ridge regression.

and Ault 1971; Rasler and Thompson 1988).

The impact of the labor force variable is significant at the .01 level. The average annual growth rate of the employed civilian labor force during the period under study is 1.8%, that is, more than two million in recent years. The addition of 150 thousand members of the armed forces to be released based on the Pentagon's most recent plan over the next *five years* to a civilian labor force of almost 115 million is therefore unlikely to have a major impact on the economy and certainly would not have an immediate effect. Cusack (1991) points out that despite the massive defense buildup of the Reagan administration, defense-related employment as a percent of the total labor force grew in the 1980s only from 5.24% to 5.68%. Other experts also agree that this is a slow process.

While these results are clearly important and we are confident about the specification of the model, the data used, and the estimation techniques, we feel we also need to share some (we think minor) weaknesses of our analysis. First, despite the use of investment and growth equations known in the economic literature to be superior to other specifications (see P. Clark 1979; Ram 1986), some of the control variables in these equations were found to be nonsignificant or not in the predicted direction. For example, the current and lagged value of nonmilitary expenditures were found to have a negative impact on private investment. This seems to coincide with the "crowding-out effect" thesis, which argues that an increase in government purchases of goods and services tends to drive up interest rates and therefore discourage investment in the private sector. But while this argument may apply to both nonmilitary and military expenditures, our analysis points to an important difference in the impact of military and nonmilitary spending on economic growth. The long-term negative effect of military expenditures on investment (equation 3) tends to persist in the economy, while the negative effects of civilian expenditures on investment (equation 3) are largely offset by their *positive* effects on economic growth (equation 5).

Second, the use of the so-called ridge regression (to overcome the problem of collinearity in the second equation), introduced a slight bias ($\hat{k}$ = .00023) into the analysis of the growth equation. Ordinary least square estimates of the original equation (which contains collinearity) produced a highly significant impact of the labor force variable on growth.

Despite these limitations, we are confident that (1) the impact of military spending on growth is indirect and (2) the impact of reduced levels of military spending on growth is not immediate: rather, it takes military spending five years to influence growth significantly via investment.

Conclusions

We have attempted to examine the timing and magnitude of potential defense spending cuts on economic growth in the United States not only by studying the direct effects of military spending but by also modeling the indirect impact of military spending (via investment) on growth. A two-equation econometric model was specified and empirically estimated using data on the U.S. economy for 1953–1987.

The findings reported in this study reveal the existence of an indirect, delayed effect of military spending on growth. To be more specific, we found that in the long run lower military spending encourages investment, which in turn promotes economic growth. This effect is not immediate, however. Our analysis shows that it takes about five years for such an indirect trade-off to begin to become manifest.

Deger points out that there exist a large number of simultaneous channels by which the effects of military spending operate (1986, 113). The causal chain we have modeled is obviously only one example for such a process. Chan suggests that increased levels of defense spending may result in higher budgetary deficits, which in turn lead to higher interest rates, which in turn reduce business investment. Likewise, reduced levels of defense spending may reduce the deficit and encourage investment (1985, 420).

Ward (1984) finds that the United States reacts positively (i.e., reduces defense spending) to decreases in the level of international tension. Those who expect the "peace dividend" to be paid immediately will be disappointed, however. If there will be a "dividend," its impact on growth will be indirect and will take several years to materialize. But better late than never.

Notes

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1. While our two-equation model is conceptually hierarchical, technically, no dependent variable in one equation appears in the same form on the right-hand side of the other equation. The use of two- or three-stage least squares estimation techniques is therefore inappropriate.

2. GLS estimates of the investment equation were very similar to the OLS estimates, except that the GLS estimate of the coefficient of the fifth lag of military spending is statistically significant at the .01 level, while the OLS estimate is significant only at the .05 level.

3. The variance inflation factors of these two coefficients are 2.43 and 2.57, respectively, indicating that the statistical insignificance was not due to severe collinearity.

4. We will provide these results on request.

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CONTROVERSIES

EXPLAINING NEW DEAL LABOR POLICY

The National Labor Relations Act (NLRA) of 1935 represented a turning point in modern labor relations policy in the United States. In the December 1989 issue of this Review, *Michael Goldfield examined the effects of worker insurgency and radical organization on the enactment of the new labor law and rejected theories that emphasized the autonomy of the state from societal forces. In this* Controversy, *Theda Skocpol and Kenneth Finegold argue that the growing strength of liberal Democrats in Congress following the 1934 election and the failure of the National Industrial Recovery Act (NIRA) as an economic recovery measure provided the most important causes for the passage of the NLRA in mid-1935. In response Goldfield argues that the results of the 1934 election were themselves influenced by the protest environment and that the passage of the NLRA was a foregone conclusion before the NIRA was struck down.*

On 5 July 1935 President Franklin Roosevelt signed the National Labor Relations Act (NLRA, or Wagner Act) into law. This measure, which had passed both the Senate and the House during the previous month, established a legally enforceable right for U.S. industrial workers to unionize by majority vote. The Wagner Act outlawed company-run unions and explicitly protected the prerogative of unions to engage in strikes. It also set up the National Labor Relations Board (NLRB) to manage representation elections and explicitly proscribed methods by which employers might interfere with workers' rights to organize autonomously.

The Wagner Act embodied the culmination of the New Deal's break with the officially repressive, antiunion policies of the 1920s.[1] Yet the Wagner Act also represented a turnaround from the industrial labor policies of the early New Deal. The Roosevelt administration's pre-1935 policy about labor organization was neither opposition to unions nor unequivocal backing for the union shop. Without creating means of enforcement, the early New Deal authoritatively encouraged all sorts of worker associations, including company unions and plural unions in single workplaces; and it also established federal agencies to mediate labor disputes as a way to avoid or settle strikes. This initial labor strategy of the New Deal, embodied in section 7a of the National Industrial Recovery Act (NIRA) of March 1933, was continued in various administrative steps taken under that law and was reiterated as late as June 1934 in Public Resolution No. 44. When it was enacted just a year later, therefore, the Wagner Act was not just a further repudiation of repression of industrial labor unions. It represented a triumph for a new approach to industrial labor disputes and unions within the evolving New Deal itself—a transition from mediation to enforcement and a move away from simple acceptance of company unions and of organizational pluralism to support for majority-elected unions independent of employer domination.

How are we to explain the passage of the National Labor Relations Act of 1935? For the last two decades, lively debates, more and more theoretically pointed and empirically refined, have developed among historians and social scientists who see the Wagner Act as a telling test case for any theory of modern U.S. politics. Criticizing arguments we have made using a state-based and electorally based approach (Finegold and Skocpol 1984; Skocpol 1980), Michael Goldfield (1989b) presents a class domination interpretation of functions of the New Deal's labor legislation, along with a class conflict explanation of the origins of the Wagner Act.[2] Goldfield's arguments are not as novel as he suggests.[3] More important, his arguments rest on selective evidence and can explain neither the contents and effects of the Wagner Act nor the process by which it became law.

Goldfield repeats the claims about the policy effects of popular insurgencies made by Piven and Cloward (1977). The chief difference is that Piven and Cloward (chap. 1–3) present the popular insurgencies of the unemployed, farmers, industrial workers, and other groups starting in the early 1930s as largely spontaneous and unorganized; whereas Goldfield feels that most of them were organized by communists and other radicals. Aside from this, the two arguments converge. Like Piven and Cloward, Goldfield portrays social unrest from below as triggering elite attempts at social control from above. Fearing the disorder and possible radicalization created by social movements from below, Goldfield maintains (p. 1274), New Deal politicians passed the Wagner Act and other legislative reforms with the clear intention "to constrain, limit, and control the increasingly militant labor movement." More precisely, he says that the Wagner Act was designed to "empower AFL unions" (p. 1274) as a strengthened bulwark against the rising threat from industrial unions, communists, and other labor radicals (see also p. 1276).

The Inadequacies of Goldfield's Analysis of the NLRA

With or without Goldfield's emphasis on the role of radical organizers, an interpretation of the Wagner Act as a deradicalizing response to a mobilized working class is inconsistent with the contents and origins of this crucial legislation. Anxious to see the NLRA as an effort to quell worker unrest, Goldfield misrepresents the plain intentions of the law. He never mentions that its promoters favored strengthened unions as a means to promote national economic recovery by raising wages and mass consumption (Fraser 1989, 69), a purpose emphasized (along with furthering industrial peace) in the law's preamble (section 1). The NLRA's sponsors believed that industrial peace could come only after the rights of independent labor unions were strengthened, a process that they realized might entail bitter conflicts with business (see Casebeer 1987, 1989; Plotke 1989). Goldfield claims that the Wagner Act was intended to avert strikes but fails to report that a guarantee of unions' rights to strike (section 13) was deliberately written into the act to avert the possibility that it could be used to outlaw strikes (see Casebeer 1989, 83). Similarly, Goldfield repeatedly asserts that the Wagner Act was meant to protect the position of the American Federation of Labor (AFL) against industrial unions yet omits to mention that Wagner and his staff turned down an AFL request to reword their bills to protect the AFL against the jurisdictional claims of emerging industrial unions (Casebeer 1989, 90).

The aftermath of the Wagner Act also belies Goldfield's account of its intentions. Senator Wagner and other key promoters and supporters of the National Labor Relations Act continued to support its approach after the strike wave of 1937 and after the NLRB became a strong promoter of the (often radical-led) industrial unions of the Congress of Industrial Organizations in competition with craft unions affiliated with the AFL (Gross 1981, 23, 49, 200; Huthmacher 1968, 233–35, 244, 259–61). Had the Wagner Act truly been designed to avoid strikes or protect conservative AFL leaders, one would have expected the act's original drafters and supporters—not just Republicans and conservative Democrats—to have sought legislative amendments or administrative changes to rein in growing industrial unions and the NLRB. But no such thing happened, because Wagner and other leftist liberals of the New Deal were truly seeking to expand the labor movement, not to "constrain and limit" labor to the established unions of 1935.

Goldfield's causal argument about why the Wagner Act passed in 1935 is equally defective. Introducing worthwhile conceptual distinctions between direct and indirect labor influence, Goldfield correctly suggests that social movements can create an overall context in which political leaders feel they must "do something" to deal with disorder. This exactly parallels earlier arguments of ours (see Finegold and Skocpol 1984, 177–78; Skocpol 1980, 187; Skocpol and Ikenberry 1983, 120–24). But then Goldfield tries to short-circuit many causally relevant factors by attributing the activities of the politicians who drafted, voted for, and signed the 1935 Wagner Act to pressure from labor and other popular insurgencies.

Quoting statements from Senator Robert Wagner and other supporters of Wagner bills about the difficulties posed by industrial strikes, Goldfield claims that during 1934 and 1935 New Dealers' worries about labor disorders meant that "they had narrow room for maneuver, usually being buffetted by events and social forces" (p. 1269). Repression of strikes could not be contemplated, Goldfield claims (p. 1275), because there was so much social unrest that federal repression would inevitably have sparked further political radicalization and possible revolution. This argument is not credible. Federal troops could have been used against major strikes either in 1934 or in 1937, as they were after the end of World War I. That such troops were not used was a political choice, congruent with the nature of the coalitions that elected and reelected Roosevelt in 1932 and 1936.

Goldfield asserts that the only conceivable alternative to repression and revolution was the Wagner Act. Thus, in the model he proposes, indirect influence from ongoing labor unrest is turned into *the cause* of the passage of the 1935 Wagner Act: "The most reasonable hypothesis to account for the passage of the NLRA is that labor militancy, catapulted into national prominence by the 1934 strikes and the political response to this movement, paved the way for the passage of the act" (p. 1273).

Goldfield repeatedly speculates about the supposed possibilities for greater radicalization and social revolution in the 1930s, but we do not have to speculate counterfactually to show that repression and the Wagner Act were not the only possible federal actions during the unrest of 1934–35. We know from the historical legislative record that these were not the only alternatives debated and tried by the New Deal politicians. The strike wave of 1934 accompanied Senator Robert Wagner's efforts to put through Congress a labor disputes bill that very closely paralleled the eventual Wagner Act of 1935; and one cannot say that Wagner devised his approach "in response" to the 1934 strikes, because the basic principles of his labor laws were well established by late 1933 (Casebeer 1989). During 1934 business leaders, Congress, and the Roosevelt administration were concerned about labor unrest; such worries did not, however, lead them to support Wagner's 1934 bill. Business leaders were vociferously opposed to the 1934 Wagner bill, as they would be to its successor a year later (Bernstein 1950, 67–68; Gross 1974, 138,

n. 141; U.S. Senate, Committee on Education and Labor 1935). Moreover, in 1934 President Roosevelt recapitulated his administration's initial approach to labor unrest by asking Charles Wyzanski, counsel to the Labor Department, and Donald Richberg, of the National Industrial Recovery Administration, to draft a new bill, Public Resolution No. 44, which he then conveyed to congressional leaders. Passed in June 1934, this law empowered the president to establish a board to investigate labor disputes; it did not give the new board any direct enforcement powers, and it did not contain the Wagner bills' substantive provisions ruling out company unions or guaranteeing labor unions majority rule and the right to strike. Leon Keyserling, Wagner's associate in drafting his labor bills, has recently said (in Casebeer 1987, 327) that "the primary objective of '44' was to derail the 1934 Wagner" bill. After Public Resolution No. 44, Senator Wagner continued to maneuver on behalf of his approach, standing fast in the face of much counterpressure from the president, officials of the Labor Department and the NIRA, and certain congressional leaders (Casebeer 1987, 305; Plotke 1989, 122).

There were, in short, sharp differences between the president and most executive branch officials on the one hand and Senator Wagner on the other about how the New Deal should relate to organized labor—differences that were articulated before the strike wave of 1934. Goldfield ignores these crucial differences. Since his vague labor insurgency model cannot account for the differences among New Deal leaders or explain the turnaround from the June 1934 Public Resolution No. 44 to the July 1935 Wagner Act, it is not causally adequate.

Why Did the Wagner Act Pass?

Strike volume declined somewhat from 1934 to 1935; and in May 1935 labor secretary Frances Perkins declared that there was "no reality to the much-advertised strike epidemic."[4] But even if one regards labor unrest (or the perception of it) as a relatively constant contributing factor, one must pinpoint factors that *changed* between June 1934 and July 1935 to explain why the second Wagner bill passed a year after the first one had failed and been replaced by Public Resolution No. 44. One needs a temporally grounded model that spells out interactions among continuing conflicts over the role of unions, electoral outcomes, sequences of public policies, and rivalries of political actors situated at different specific locations within the federal government.

There were two critical changes between mid-1934 and mid-1935. First, breaking with the usual pattern in which an administration in power loses congressional seats in an off year, the fall 1934 elections substantially strengthened liberal Democratic ranks in Congress, while weakening Republicans and Democratic conservatives. Redistricting had created more urban seats in Congress, and working-class ethnic voters were increasingly turning out for the Democratic party (Andersen 1979; Brown 1988; Key 1955; Sundquist 1983, 198–239). Shifts in electoral politics, not increases in workplace militancy, were thus what heightened "labor influence" on legislation—and liberal influence more generally—between 1934 and 1935. Wagner and his allies could be more confident in forcing their labor bill to a vote in 1935 even in the face of presidential reluctance to endorse it (see Casebeer 1987, 305).

Second, the early New Deal's keystone policy for economic recovery, regulating industry, and dealing with business—the National Industrial Recovery Act—met its demise between 1934 and 1935. The NIRA was ruled unconstitutional in May 1935, and even before this legal coup de grace it had become obvious to business leaders and to the Roosevelt administration that the NIRA was not a successful economic recovery measure. Just as Roosevelt had to face strengthened, independent-minded liberal forces in Congress after the fall of 1934, he came to a bitter impasse with business leaders and found himself without an official economic recovery strategy. The NIRA's demise also undercut Hugh Johnson and Donald Richberg, two of the most vociferous Roosevelt administration opponents of Wagner's approach to unions; and this gave Wagner and his labor board allies broader leeway to argue within Congress and executive circles in favor of their bill. In the vacuum created by the end of the NIRA, they presented the NLRA as a good way both to promote economic recovery and to put labor relations on a new footing. Congress passed Wagner's second labor bill in 1935, and President Roosevelt finally decided to accept it after it had gone through the Senate and was pending in the House.

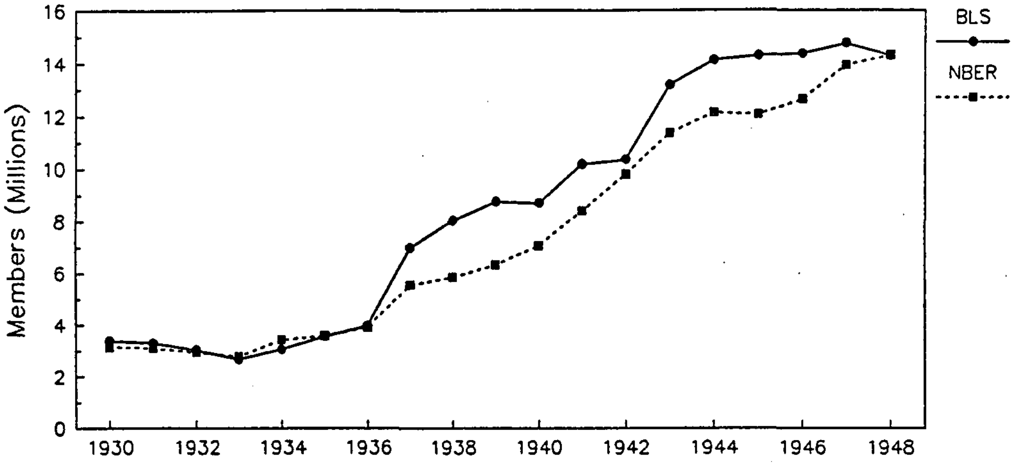
Our model also highlights the larger institutional contexts and longer-term processes that connected the Wagner Act to earlier, unsuccessful New Deal policies (invoking variables that are not encompassed in Goldfield's Figure 1). We especially point to the playing out within society and government of the unintended effects of the National Industrial Recovery Act of 1933. The enduring administrative weaknesses of the U.S. national government, the federal executive's overdependence on trade associations and business corporations to implement economic policies, and the institutional capacities of the courts and Congress to check or get around executive policy initiatives—all of these institutional features of the state in relation to the capitalist economy ensured the failure of the NIRA as an economic recovery strategy and as a method for mediating labor disputes. The Wagner Act would not have been possible had the NIRA succeeded—embodying, as it did, a rapprochement between the Roosevelt administration and sectors of business that favored administrative price fixing and either company unions or no unions at all.

Yet even as it failed, the NIRA spurred social and political forces that led toward the Wagner Act. Section 7a of the 1933 law emboldened labor organizers without constraining opposition to unions, thus feeding industrial unrest. What is more, Senator Wagner and virtually all of the lawyers and economists he worked with in drafting successive versions of what became the National Labor Relations Act were members of, or directly concerned with the problems of, the various labor boards established under the NIRA. The specific ideas that went into Wagner's labor bills can be traced to their drafters' frustrating efforts to implement NIRA labor mediation policies. The NIRA labor boards provided the precise context within which actors within the state tried to cope with labor unrest and especially with the unwillingness of most businesses to accept independent labor unions. Initial failures and frustrations under the NIRA led these official actors to believe that more state building had to be done—that is, it led them to formulate the strong substantive legal guarantees for independent unions and the means for federal enforcement against business resistance that made the Wagner Act an innovative departure within the New Deal as a whole. The Wagner Act was the product of administratively situated policy learning, because, as Leon Keyserling has aptly put it, "section 7(a) stirred things up but couldn't be enforced" (Casebeer 1987, 301).

Why Did Workers Mobilize?

Underlying Goldfield's characterization of New Deal labor legislation is his vanguardist conception of labor mobilization. For him, class formation depends on the efforts of radical activists. The state

Figure 1. Labor Union Membership, 1930–1948



can only retard—not to any significant extent promote—working-class organization. This conception leads Goldfield to belittle the impact of the Wagner Act and its predecessor, section 7a, on the labor mobilization of the 1930s and to exaggerate the role of communists in this upsurge.

As evidence of the irrelevance of the Wagner Act and the NLRB to working-class organization, Goldfield points to the "stagnation" of the labor movement in the late 1930s. But this interpretation is an artifact of questionable statistics. The Bureau of Labor Statistics (BLS) estimates of union membership on which Goldfield relies (p. 1267; from U.S. Department of Labor 1980, 412) are based on union self-reporting and thus are distorted by the inflated and unstable claims of the Congress of Industrial Organizations (CIO) in the first years after secession from the AFL. In fact, BLS estimates for CIO membership in 1939 and 1941 are based solely on the claims made in convention addresses by CIO presidents John L. Lewis and Philip Murray (U.S. Department of Labor 1950, 139; CIO 1939, 80; CIO 1941, 162). These figures are not substantiated, are not disaggregated by individual unions, and are suspiciously well rounded numbers: four million for 1939, five million for 1941.

More reliable for this period are the membership estimates from dues receipts calculated by the National Bureau of Economic Research (NBER); these data continue the Wolman series ending in 1934 that Goldfield uses for earlier estimates (Troy 1965). As Figure 1 shows, although the BLS estimates depict a stagnant or even declining unionization movement after 1937, the NBER estimates display continued growth. The U.S. labor movement became stagnant only in the 1950s, after the gains of the New Deal and the war period and after the genuinely repressive Taft-Hartley Act of 1947.[5]

Goldfield's effort to minimize the positive impact of New Deal labor legislation is equally mistaken with regard to section 7a. Both the BLS and NBER data reveal that union membership declined with the onset of the Depression, then increased from 1933 on, despite the sluggishness of economic recovery. The importance of 7a in stimulating labor's efforts to unionize is evidenced by the remarkable timing of the upturn in unions and by regression estimates in a recent quantitative study

(Wallace, Rubin, and Smith 1988, 18–23; see also Fraser 1989, 68; Valelly 1989, chap. 7). The importance of 7a has also been attested to by all sorts of participants in the original events—including public officials (Johnson 1935, 344–45), anti-union employers (Weir 1934, 82), and mobilized workers (Korth and Beegle 1988, 82). Union activists deliberately interpreted section 7a as saying to workers, "You can organize, the government *wants* you to organize" (Lynd and Lynd 1973, 158). And workers responded—even though President Roosevelt and National Recovery Administration (NRA) administrator Hugh Johnson were actually quite ambivalent about independent trade unions, while the more sympathetic NRA labor boards lacked independent powers to enforce their prounion views.

Even as Goldfield places too little emphasis on the role of legislative encouragement for labor mobilization in the 1930s, he credits the communists with too much influence. Goldfield seems (pp. 1270–73) to treat communist participation in protests and strikes as the major cause of social unrest (and hence, in his overall model, of labor legislation). Although communists (and other radicals as well) can be credited with courage and innovativeness that gave them influence well beyond their numbers, it is well to keep in mind how little support the communist party had in the early 1930s. Party membership was 18,119 in 1932; and the communist presidential vote, though double that of 1928, was less than one percent of the national total (Klehr 1984, 88–89, 91). Moreover, the communists' separatist Trade Union Unity League peaked at 125 thousand members in 1934, less than 4% of all union members at that time (U.S. Department of Labor 1936, 15; U.S. Department of Labor 1950, 139).

In many ways, the Communist party was less the source than the beneficiary of labor legislation and union mobilization in the 1930s. Communists organized within mass production unions, such as the United Auto Workers, that were founded in response to the promises of section 7a. The NLRB established by the Wagner Act initially favored CIO unions, where communists were most active (Valelly 1989, chap. 8); indeed, the NLRB's legal staff included members of a secret communist cell (Gross 1981, 131–50). The high point of Communist party membership, at eighty to ninety-thousand, came in 1938 (Klehr 1984, 366–67), after the Wagner Act had been upheld in the courts and was being fully enforced but before the divisive effects on the party of the Nazi-Soviet Pact. Goldfield suggests that without the NLRA, the labor movement of the 1930s would "certainly" have developed "in a more radical direction" (p. 1278). More likely, New Deal labor legislation culminating in the Wagner Act helped to make industrial workers more amenable to radical organizers than they otherwise would have been.

Theoretical and Methodological Implications

Michael Goldfield accompanies his account of the Wagner Act with broader appeals that we can endorse with certain additions and qualifications. He calls for precise modeling of causal processes in politics and rightly says that hypotheses and models have to be tested against both available evidence and theoretical competitors. We can only add that good models and causal hypotheses need to be tested against concrete historical *variations* in outcomes. Scholars should not just present stories based on selective descriptions and counterfactual speculations but actually account for variations in specified types of outcomes. Had Goldfield held himself to this standard, he could not have put forward the arguments he did. His model fails to explain not only the contrasts between New Deal labor laws in 1934 and 1935 but also, for example, the interactions of U.S. labor mobilizations with governmental policy making in the 1930s, as compared to the interactions leading up to 1919, when President Wilson used federal troops against strikes in coal and steel, and the interactions of the mid-1940s, when a postwar strike wave led to passage in the following year of the Taft-Hartley Act (which attached important antiunion provisions to the NLRA).

Goldfield argues that the effects of industrial labor actions on public policy outcomes may be registered in different ways. When labor cannot directly determine state actions through measures taken by political parties that hold governmental power, labor may exert influences by other means. Strikes or broader unrest may help to bring about initiatives "by a government in order to stem working class militance and organized radicalism" (p. 1265). Or (although Goldfield does not mention this possibility) labor may amass sufficient electoral power as a minority within established parties to encourage government to take its interests into account in some way. All possible alternative forms of labor influence do need to be explored. (They have been, for example, in the extensive cross-national literature on the development of welfare state policies.)

Yet an important caveat needs to be voiced about Goldfield's notion that analysts can expect to find straightforward relationships between militant labor unrest and governmental "concessions" (as he calls them). In none of the historical examples that Goldfield cites (p. 1265)—the welfare reforms of Bismarck's Germany in the 1880s, the recent policies toward black trade unions of the white South African regime, or the labor legislation of the New Deal—were governmental authorities in a position where they "had to" respond to labor unrest with only one sort of "concession." In all of these cases alternative responses were possible, including repression and various sorts of reforms. The policy responses actually made depended on the consequences of prior policy attempts; the capacities of administrative and political institutions to devise and implement measures; and on shifting ideas, political alliances, and balances of power among economic and political elites. Unrest from below may have pressured governments to do something. But the sheer fact of that unrest cannot explain why these governments did what they did when they did it.

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There are at least two fundamental differences between Skocpol and Finegold and myself on the National Labor Relations Act. The first concerns why the bill passed the Congress in 1935. The second is about the role the NLRA played in the subsequent growth of the labor movement. I shall start with a discussion of the first question.

A major purpose of my analysis has been to explore the reasons for the passage of the 1935 NLRA. I emphasize (1) the growing strength of the labor movement (only two of whose indicators are increased strikes and union membership growth); (2) the magnification of labor influence due to the broader social protest milieu, which included widespread militant, radical-led social movements and numerous local and state-level third parties; and (3) the growing strength of radical organizations, especially the Communist party, within this environment. These factors, I argue, are crucial for explaining the passage of the National Labor Relations Act and make the 1930s significantly different from the 1919–20 and 1945–46 periods, when strike militancy alone was arguably equal or greater. Skocpol and Finegold, in contrast, downplay the importance of this reality, while stressing the supposedly causally determining role of independent reformist intellectual agendas and a broad range of governmental and other phenomena, all mistakenly subsumed under the umbrella of state activity. Their interpretation, I believe, is fundamentally flawed.

1919 Versus 1935

Skocpol and Finegold note that "labor mobilization" (by which they presumably mean strike militancy) was at least as large in 1919 (and I would add 1945–46) as during the 1930s. Yet government response was less favorable. They suggest that autonomous state response, not labor militancy, is thus the key explanatory variable. The difference between the protest environments of the 1930s and the other two periods, however, is quite stark. 1919 saw tremendous strike militancy located in a number of geographically concentrated industries (particularly steel, coal, and meat packing). There was no general economic and social crisis in the country as in the 1930s. The 1930s, in contrast, had brought wide strata of the population, in addition to factory workers, into militant protest activities. Initial repression by the government and corporations had led to the growing influence of radical organizations. In 1919 radical influence was uneven, not necessarily working within established unions. In 1946 it was declining and already on the defensive. These are crucial aspects of my model. I have cautioned against making a fetish of strike statistics (or any other quantitative data for that matter) and using them without reference to their historical context, a caution Skocpol and Finegold choose to ignore.

This different historical context makes the legislative environment of the 1930s distinct from the other periods. Thus, a focus on the factors that were specific to the 1930s is appropriate in determining the reasons why the 1935 NLRA passed the Seventy-fourth Congress. Skocpol and Finegold see Senator Robert "Wagner and his allies" as autonomous state actors, or elites, who were the primary causal agents in the passage of the NLRA. These actors took advantage of a changed context; and the influence of labor militancy, broad social movements, and the growing strength of radical organization was at best secondary. Skocpol and Finegold cite two "critical changes between mid-1934 and mid-1935" that define this new context: the November 1934 midterm congressional election and the overturning of the National Industrial Recovery Act by the Supreme Court in May 1935 (the Schechter case).

The 1934 Election

Let us turn to the 1934 midterm election. This was indeed an extraordinary election.[6] It was the only one since the Civil War (with the exception of the 1902 congressional election when Theodore Roosevelt had just succeeded the recently assassinated president William McKinley) in which the presidential party increased its majority in an off year. Even more important was the increase in left-wing Democrats, progressive Republicans, and radical third-party congressmen and senators. This unusual victory and the election of a more left-leaning, "pro-labor" Congress, however, cannot be examined merely as an exogenous variable. Rather, the outcome of the election was in good part the result of the activities of broad social movements among the urban unemployed, farmers, Afro-Americans, and others and of the 1934 labor upsurge. In addition, the new composition of Congress was a consequence of the organizing of dozens of powerful state

Table 1. Seats Won in the Congressional Elections of 1930–36

Congressional Party	1930	1932	1934	1936
House of Representatives				
Democrats	216	313	322	333
Republicans	218	117	103	89
Other	1	5	10	13
Senate				
Democrats	47	59	69	75
Republicans	48	36	25	17
Other	1	1	2	4

Note: After the 1934 elections the House included 13 Progressive Republicans, 3 Minnesota Farmer-Laborites, and 7 Wisconsin Progressive party candidates. The Senate included 10 Progressive Republicans, 1 Farmer-Laborite, and 1 Progressive.

Sources: Harold W. Stanley and Richard G. Niemi, *Vital Statistics in American Politics*, 2d ed. (Washington: Congressional Quarterly, 1990), 113; James Patterson, *Congressional Conservatism and the New Deal* (Lexington: University of Kentucky Press, 1967).

and local third parties, as well as of the activities of a number of broad, unorthodox protest movements, including Huey Long's Share the Wealth movement. All these factors are stressed in my article. The results of the 1934 election are inexplicable without direct causal reference to the social protest environment. Thus, the 1934 elections cannot be understood merely as an independent variable affecting the policy context or environment. They must be seen as an intervening variable that in good part reflected the Depression-inspired demands and attitudes of the vast majority of the population. This point, I should note, seems to me to be unexceptional, acknowledged by all reasonable scholars. Thus, a full analysis of the 1934 congressional elections would only serve to highlight the influence of the factors stressed in my original article.

Yet the new numerical composition of the Seventy-fourth Congress is not by itself sufficient to characterize the new "policy environment." A brief look at some figures will help to indicate this point. As can be seen in Table 1, the biggest numerical change came in the 1932 presidential election. One must be cautious, however, about directly translating party affiliations and ideological proclivities automatically into votes and support for new policies. This caveat is especially true during times of upheaval and stress. As James Patterson emphasizes in his analysis of election results in 1932 and 1934, "Statistics cannot recreate, however, the powerful demand for government action to alleviate distress" (Patterson 1967, 12). And in analyzing the case with which the initial New Deal legislation of the first hundred days in 1933 passed (in an argument that could be paralleled for the passage of the 1935 NLRA), Patterson argues: "All of these assets . . . Roosevelt's numerical majorities in Congress, his popularity, charm, tactical ability, and handling of patronage—profoundly affected congressmen. Yet these advantages were secondary compared to the influence of the Depression. In 1937, for example, Roosevelt had larger congressional majorities, no less charm, a great deal more experience, yet Congress gave him next to nothing in a long and bitter session" (1967, 11).

And what applies to President Roosevelt's influence applies all the more to "Wagner and his allies."[7] My only addition to Patterson's remarks would be to add that as part of the general explanatory variable *Depression*, one must include the strength and influence of the social movements. Thus, one must look both at the factors that led to the outcome of the 1934 elections (the election of such a left-wing Congress) and also to the dynamic factors that made this particular Congress so responsive to the influence of labor insurgency, social movements, and radical organizations. It will not do simply to look at the 1934 election and the Seventy-fourth Congressional legislative session apart from the dynamic social context in which they existed. Thus, a consideration of the 1934 elections, urged by Skocpol and Finegold, only serves to strengthen my original argument.

The importance of outside influence on Congress is highlighted by comparing the Seventy-fourth Congress with the Seventy-fifth (elected in 1936). The Seventy-fifth Congress, as the above quote from Patterson implies, passed little progressive and prolabor legislation. Yet its numerical composition would suggest it would be highly amenable to such legislation. This is a multifaceted question worthy of a detailed examination; it is not possible to do full justice to this subject here. Several factors would have to be included in any careful discussion. Among the reasons for the decline of labor influence and the development of a more effective organized conservative opposition in the Seventy-fifth Congress was, first and foremost, the deep split within the organized labor movement, which reached heights described by some commentators as "fratricide" and as labor's "civil war" by late 1937 and early 1938. In 1935 this split had not surfaced. The industrial unions had not left the American Federation of Labor; the CIO had not yet been formed. This change was the most important factor in labor's more limited influence within Congress. The split was so severe that many of the more conservative AFL craft unions were led to ally with Republicans and Southern reactionaries in their battles with the more radical, industrially organized CIO. Second, the recession, beginning in late 1937, also played a role in slowing labor organizing. In this context, the Southern defections from the coalition that supported the NLRA (after the dramatic 1937 sit-down strike wave) were of special importance.[8]

In trying to make some sense of their argument, one might note the claim that redistricting might have had something to do with the results of the 1934 or the 1932 election. This argument also finds little empirical support. Significant redistricting did take place after the 1930 census. (One could, of course, argue that this was a result not merely of state activity but of demographic shifts and economic development.) An examination of congressional district maps for each state in the appropriate *Congressional Directories* suggests that virtually all redistricting was done before the 1932 election; only Texas changed its districts between the 1932 and 1934 elections. Thus, redistricting can have little bearing on the changes between the two elections. The majority of states did redistrict after the 1930 census. Yet a cursory look suggests that Democratic majorities in both the presidential and congressional races were as great in the states that did not redistrict as in those that did. I prefer Sundquist's explanation: "When unemployment stands at 24 percent, as it did in 1932, an incumbent president is not reelected" (1983, 210). Nor, one might add, are many of the congressional members of his party.

The Overturning of the National Industrial Recovery Act

Let us briefly consider the second factor that Skocpol and Finegold see as important for the new context that emerged before the passage of the NLRA, the striking down of the National Industrial Recovery Act by the Supreme Court in the Schechter case on 27 May 1935. While the overturning of Roosevelt's recovery program may have been one important influence in his belated decision to support the NLRA (after overwhelming support was already emerging in both houses of Congress), it came far too late to play a role in either the House or Senate deliberations. The Senate, in fact, had already passed the NLRA in final form 63 to 12 on 16 May, eleven days before the Schechter decision was delivered. The die had already been cast; even in the House the final vote was all but certain by the time of the *Schechter* decision. That this factor is emphasized as central to the passage of the NLRA is further testimony to the weakness of Skocpol and Finegold's argument.

Strike Waves and Elementary Statistics

One key issue in the debate over the passage of the NLRA is the timing of the developing strike wave in the early and middle 1930s. To what degree did the wave or perceptions of it lead to the passage of the act? Skocpol and Finegold raise this question again in their rejoinder. I, of course, in my article emphasized major problems with their analysis of strike data. Thus, it is worth looking at strike statistics for the first half of the 1930s. If one wants to talk about these things in a serious and responsible manner, one must actually look at the data. Skocpol and Finegold claim that "strike volume declined somewhat from 1934 to 1935." They do not define what they mean by strike volume. The number of strikes in 1935 (2,014) was greater than in 1934 (1,856), although the number of days idle was greater in 1934. Yet they wish to focus on the period just prior to the passage of the act. During the first three months of 1935, the number of strikes was substantially higher than in the first three months of 1934.[9] It is thus hard to know what to make of this undocumented assertion about the decline of strike militancy before the passage of the NLRA, an assertion that figures prominently in the present reply and in their other published works. As I suggest in the article, the one period prior to the passage of the NLRA when strike rates did actually decline was in the later months of 1934. Yet such a decline is merely part of the well-documented cyclical (and seasonal) pattern of strike rates that is reflected in all strike data not just in the United States but in other countries as well for all historical periods; hence, it cannot be relevant to the argument (Keenan 1986).

As I also argue, it is not enough to hang one's hat on mere statistics. The 1934 strikes were truly dramatic and earth-shaking, as virtually all historians and contemporary observers have contended. I have presented a wide sampling of these assessments in the article. Skocpol and Finegold's quote from Secretary of Labor Perkins is a bit perverse, actually serving as further evidence for my assessment, not theirs. There was in fact a "strike epidemic" that was much advertised, reported, and accepted as fact by virtually everyone (as the quote from Perkins suggests), leading to a groundswell of support and pressure for the passage of the NLRA. Perkins, however, did not want a National Labor Relations Board (as proposed by the act) established outside of the Labor Department (her bailiwick). She thus had institutional reasons for trying to downplay the "strike epidemic" and thereby undermine the rush to pass the NLRA, particularly in its unamended form. Though Frances Perkins was an important member of the cabinet and one of Roosevelt's closest advisers, her position had no effect on the content of the bill; the pressure to pass the bill in unamended form was much too powerful. Thus, Perkins's remarks have the same ring as those of Chicago Mayor Michael Bilandic when the City of Chicago was immobilized during the blizzard of 1979. Bilandic, who was about to be swept out of office by an obscure candidate in the mayoral election, kept appearing on television, telling an incredulous populace that everything was under control, that there was no snow emergency. The moral of both stories is that saying it doesn't make it so.

Another important point mentioned in my article—ignored by Skocpol and Finegold but worth mentioning here—has to do with the timing of the labor upsurge and the passage of the bill. Even if strikes had declined some between 1934 and 1935 (as Skocpol and Finegold suggest they did), this would not be a compelling argument against the importance of the influence of labor insurgency. Strike frequency declined during the late 1920s, maintaining relatively steady, but low rates from 1927–32. In 1933 they doubled, dramatically rising again in 1934. The influence of social movements, however, on public policy is rarely immediate (short of revolution, a claim I have never made about the 1930s, contrary to Skocpol and Finegold's assertions). Often there is a considerable time lag. I gave the example of South Africa, where a massive upsurge of workers took place between 1973 and 1975. Reforms legalizing African unions were not instituted until 1979, long after strike militancy had died down. The importance of this example, seemed to me to be that reasonable people would be less likely to downplay the influence of South African workers within the reform process, since the autonomous strength of reformers within the Nationalist party could not be presumed to have been large. Time lags are a common feature of many social science models. I argue for their importance in a causal analysis of the passage of the Wagner Act. The NLRA was not merely a response to particular events just before its passage but to an increasing wave of social protest and the rapidly expanding role and influence of radical organizations within it, a process that had been developing for a number of years. I will not belabor this point here, since it is discussed in detail in the original article. I take this point, however, as another example of how the theory of state autonomy leads one to narrow the view of politics to exclude relevant societal and political influences, including the arbitrary limiting of causal influence over time, to the detriment of reasonable social-scientific explanations.

The Impact of the NLRA on Union Growth

A further major issue dividing my argument from that of Skocpol and Finegold is the role of the NLRA in contributing to the 1936–37 massive industrial union drive. The language that they use to describe the supposed effects of the 1935 act bears no relation to the actual role it played. At the very beginning of their reply they talk of the enforcement powers, the establishment of "a legally enforceable right for U.S. industrial workers to unionize." They talk about what the Wagner Act "outlawed" and "explicitly proscribed." Yet until the NLRA was upheld by the Supreme Court in April 1937 in the Jones and Laughlin case, it was universally disobeyed by corporations and thus went unenforced. The evidence shows that not only was there no enforcement of the NLRA but little use of it by unions in the two years after its passage. Meanwhile, millions of workers were unionized with negligible involvement with the National Labor Relations Board. A more extensive argument appears with data in the original article. Yet Skocpol and Finegold make no mention of the facts and figures I present, which suggest the limited role played by the NLRB and also contradict their own published assertions. Repeating assertions over and over again (while supplying no evidence for them) does not make them so. One must carefully look at the data and statistics.

Now, I believe that the NLRA was extremely important in determining the type of union growth that was to take place and the structure of labor-management relations from the 1930s until the present. But this is a separate issue from the degree to which the NLRA stimulated union growth during the 1930s. As we have seen, the state autonomy theory has special difficulties in explaining the enormous union growth before the act was upheld. It is an even more severe problem for such a theory that union growth stagnated just as the act was becoming effective, thus suggesting just how little impact the NLRA had on union organizing at the time. Skocpol and Finegold respond to this problem with an extremely bold move: they deny that union growth stagnated at all. To make this argument they have "discovered" a new set of statistics that supposedly bolsters their argument against my analysis. They claim that the NBER union membership series is superior to the BLS series, particularly for the 1930s. They further argue that the NBER series shows that union growth did not stagnate after 1937, that the "U.S. labor movement became stagnant only in the 1950s." First, I find their discussion of union membership series odd. All union membership series for the United States are biased; each has its pros and cons. I discuss this in detail (comparing the BLS and NBER series) in my book *The Decline of Organized Labor in the United States* (1989a), mostly in the footnotes (see, e.g., pp. 8–20). I conclude there that "union membership data for this country should in general only be considered an indicative" (p. 8). The NBER series, as Skocpol and Finegold note, is based on dues receipts by individual members for a whole year. Its bias is toward underreporting, particularly during times of new organizing. The reason is that it takes some time after they are organized (and counted as members by the union) before new members become stable dues payers. If the union is new (as were many of the CIO unions), it may take even longer before the union is transformed into an efficient dues-collecting operation and new members are regular subscribers. In times and places when dues payment is voluntary (as it was during the 1930s), there will be an additional substantial lag between the organization of new members and their appearance on regular-dues-paying lists. Thus, the bias of the NBER series is twofold. During periods of dramatic increases in new organizing (as in 1936 and 1937) it understates the number of new members organized. In a period when this new organizing is tapering off or stagnating (as in 1938 or 1939), the stagnation is understated because many previously organized workers are only then appearing on dues ledgers. These biases tend to underemphasize (smooth over) both dramatic increases and subsequent periods of stagnation. Thus, which series is "more reliable" and for what purposes is highly debatable. And here it is undoubtedly appropriate to comment on the data that they use in their 1984 article (cited on p. 1266 of my original article): their argument there about the importance of the NLRA for subsequent union growth clearly uses the more dramatic BLS figures, not the NBER data that they now extol as the most reliable. This might lead an objective observer to one of two conclusions: either their earlier (quite strong) remarks were based on data they now find unreliable, or else they alternately use the data sets that best serve their immediate purposes. If the former (more charitable) interpretation is the case (as I certainly assume it is), an admission of error on their part might be expected.

There is no need, however, to choose among data sets or speculate on the stagnation of union organizing, particularly among industrial workers in the period from late 1937 to 1939. One need not make a fetish of the numbers. There are detailed historical accounts of the period that leave little doubt that there was such a stagnation. If Skocpol and Finegold have some *new* evidence to add to the voluminous scholarship of this period, it would help us all if they would cite it. One could amass a large dossier of evidence to support what everyone else but Skocpol and Finegold have discovered. Walter Galenson summarizes this period as follows: "The year 1938 was not a good one for trade unions" (1960, 44). Art Preis states unequivocally that "by September

1937, the heroic days of the CIO were over" (1964, 72). Both give extensive figures and documentation. While there are clearly minor exceptions and nuances, the overall trend during this period is clear. Organizing in the automobile industry came to an abrupt halt; the Ford campaign, in particular, got nowhere. No dent was made in steel as intransigent resistance from the Little Steel companies (much of it in violation of the Wagner Act) kept their plants nonunion. Similar stories can be told in virtually every industry, including rubber, electrical, textile, lumber, oil, the building trades, and among machinists. Galenson and Bernstein, among others, provide extensive, union-by-union and industry-by-industry documentation (e.g., Galenson 1960, 115, 157, 253, 261, 278, 336, 392, 417, 512, 520; Bernstein 1969, 562, 601, 620-21). Additional documentation may be found in Derber (1957), Keeran (1980), Lichtenstein (1982), Milton (1982), and Stolberg (1938). In fact, one can even glean much of this story by carefully looking at the NBER union membership data set, allowing for its known biases. And here it is perhaps only fair to note the illusion created by looking at Skocpol and Finegold's Figure 1 (union membership graph). The graph is scaled in such a way so that a rise in union membership from less than 3 million to 14 million members appears as a gentle slope, the dramatic rise from 1933 to 1934 hardly a blip on the paper. It is more instructive to look at the actual figures. The NBER series has an increase in union density from 1936 to 1937 of 4.4% (13.5%-17.9%); an increase from 1937 to 1938 of 2.1% (17.9%-20.0%), most of which was in the beginning of 1937, in the period immediately after the auto sitdown strikes, which ended in late January; an increase from 1938 to 1939 of .7% (20.0%-20.7%); and an increase from 1939 to 1940 of 1.1% (20.7%-21.8%). Given the biases of the NBER series, it is clear that the stagnation is captured even here. Any serious social-scientific examination of the question of union stagnation (particularly a claim such as Skocpol's and Finegold's, which goes against all the careful scholarship of the period) ought to weigh the statistical evidence carefully and circumspectly, delving in addition into the rich historical, institutional literature on unions during this period. It is indicative of Skocpol and Finegold's approach that they do not do this. A careful examination is necessary because their analysis has been widely accepted among serious social scientists who are not specialists in this period. Acceptance of the state autonomy arguments on the Wagner Act as good coin of the realm inhibits more serious, careful arguments. Thus, the weaknesses of their argument must be brought to light if more reasonable and serious ones are to gain a fair hearing.

Additional Problems

Many of their arguments so caricature my positions that it is probably best not to try to correct them here. For those who wish to know what I actually say, I can only recommend the reading of the original article.[10] For example, I never say—nor do I believe—that "the state can only retard . . . working-class organization." I do not "belittle the impact of the Wagner Act," only try to place it in proper perspective. I try not to "exaggerate" the role of communists, only to give them their due. There was a tendency in Cold War labor history to belittle or write off the role that communists and other radicals played in labor organizing during the 1930s (see Goldfield 1980, 1985). Fortunately, more objective accounts are available now in the form of hundreds of articles, theses, and books published in the last two decades. Skocpol and Finegold seem unaware of this scholarship and would do well to consult it. It is interesting that those who think that the actions of political leaders and bureaucrats are often so decisive should be so quick to write off the possibilities that different types of organizers and leaders might have differing impacts on mass movements.

The whole house of cards of the state autonomy approach seems to come tumbling down with the resort to the argument presented in their final paragraph. They argue that governmental authorities never " 'had to' respond to labor unrest with only one sort of 'concession.' " Referring to my examples of social welfare reforms in Bismarck's Germany and labor union legalization in South Africa, as well as to the NLRA, they assert, "In all these cases alternative responses were possible." I believe that this argument approaches a reductio ad absurdum of their position. They have excluded a priori any possibility that any type of labor activity could be central, whatever the empirical evidence. People never "have to" do anything (except, as the old adage goes, die and pay taxes—and even the latter is sometimes avoided). Let us take an extreme example. A large, physically imposing thief puts a gun to the victim's head and demands, "Your money or your life!" It is, of course, true that the victim does not "have to" hand over the money. She or he has numerous other alternative courses of action including fighting back, screaming, spitting in the thief's face, refusing, dying, and so on. Now my argument is not that this hypothetical situation bears a strong resemblance to the role played by labor influence during the 1930s; I have in fact argued that it is more complex and subtle, certainly not (despite Skocpol and Finegold's distortion of my argument) that there was an immediate revolutionary threat to the government or to capitalism. Yet the attempt to argue that there are always alternative responses possible is especially revealing about their method of analysis. By the same argument, one could just as easily dismiss the importance of the gun in my example. Any reasonable causal account, however, of why the victim did in fact hand over the money would be seriously remiss (and not very good social science at that) if it did not stress the causal role of the gun despite the fact that the victim did not "have to" do what she or he did, having numerous other alternatives. Yet if the method of analysis preferred by state autonomists cannot provide an adequate account of so stark an example as that given above, what hope does it have in aiding us in understanding and explaining the more complex world of politics?

Conclusion

As I argue in my original article, theories that are inadequate or inferior to their competitors are often discovered to be so because their proponents are forced, in defending the theories, to omit important facts, to distort reality, and to make far-fetched arguments. This is the dilemma faced by state autonomists.

Of course, we must pay attention to the motivations, histories, and intentions of political actors; to the struggles within governments and bureaucracies; and even to developing state capacities. But we should not engage in such investigation in isolated fishbowls. The importance of societal forces, social movements, and class relations in influencing, shaping, and sometimes dominating state activities must be given its due. Pluralists made a significant advance over the old conservative elite theories by stressing the importance of societal influence on the political process. Although it is fair to argue that they did not go far enough—and I would certainly take this tack—those who would ask us to return to the older view of state activities as isolated, autonomous processes carry a heavy burden of proof.

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Notes

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1. Repression of unions remained the rule for agricultural workers, however, who were not covered by the Wagner Act.

2. Glossing over important differences among neo-Marxist writers, Goldfield accuses us of not having a correct understanding of Marxist theory of the New Deal! Goldfield (pp. 1262–64) mistakenly attributes to us simplified functionalist arguments about political influence that were actually made not by us but by particular neo-Marxist authors we were empirically criticizing.

3. Goldfield joins others in asserting that New Deal labor legislation had a deradicalizing effect on labor insurgencies (see Levine 1988; Piven and Cloward 1977; and Tomlins 1985; for a convincing refutation of these assessments, see Plotke 1989). Goldfield also signals (pp. 1263, 1269, 1278) that he agrees with those who have previously argued (cf. Domhoff 1970 and Ferguson 1984) that the New Dealers who drafted the Wagner Act were agents of certain capitalists or of their tame allies in the leadership of the American Federation of Labor.

4. Quoted in the *New York Times*, "Strikes Minimized by Miss Perkins," 20 May 1935. Perkins characterized the strikes as a normal accompaniment of economic recovery.

5. Goldfield (p. 1268) inaccurately cites Weir and Skocpol (1985, 112) as endorsing the notion that the labor movement stagnated in the later 1930s; actually, they were referring to the 1950s in this passage.

6. These elections are mentioned briefly in note 20 of my *APSR* article; I welcome the chance to expand on those comments here. As my remarks will suggest, it is hardly possible to consider elections merely under the rubric of state activity.

7. Skocpol and Finegold have a tendency to confuse and blur quite different questions. One question concerns the legislative history of the NLRA, including who drafted it and what their intentions were. This story is told fully and quite satisfactorily by Bernstein (1950) and Casebeer (1987, 1989). The second question—the one on which I focus—is why the bill was successfully passed. I should stress that the accounts of Bernstein and Casebeer concerning the first question are completely compatible with my model of why the bill passed.

8. In my forthcoming book on the 1930s and 1940s, I particularly stress the importance of conservative Southern elites, the race question, and the impact on U.S. politics of the failure of labor organizing in that part of the country.

9. The comparable figures for January, February, and March of 1934 are 98, 94, and 161 respectively; for 1935, they are 140, 149, 175 (see selected issues of the Bureau of Labor Statistics' *Monthly Labor Review*). Number of strikes per month are the only published monthly strike data available for these years. Detailed analysis of these and other labor data for this period will be presented in my forthcoming book on the politics of the 1930s and 1940s.

10. It is, of course, easy to distort or caricature an opponent's position. Several years before my article was published, I sent drafts to Skocpol, Finegold, and other state autonomists, asking for comments, and in particular, asking if they felt I had distorted their arguments in any way. I also asked several people sympathetic to their position the same questions. In several conversations and written communications with Skocpol, no question of any distortion arose. Thus, I find present claims that I misread their argument in some minor ways to be unconvincing. I am particularly sure that my criticism of their distortion of Marxist arguments (alluded to in note 2 above) is on the mark. For a more extended analysis of tendencies in this latter direction by state autonomists see Cammack 1988.

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PLATO'S PARADOX? GUARDIANS AND PHILOSOPHER-KINGS

For centuries scholars have engaged in interpreting the meaning of Plato's Republic. *In this exchange, Peter Steinberger and Christopher Duncan debate the role of guardians and philosopher-kings in the ancient city. This controversy is ignited by Steinberger's essay on Platonic rulers in the December 1989 issue of this* Review.

In his essay on "Ruling: Guardians and Philosopher-Kings" Peter Steinberger attempts to point out what he considers a serious paradox in the structure of Plato's *Republic,* namely, the logical inconsistency inherent in the notion he ascribes to Plato that a ruler must be both a philosopher and a guardian. He argues—quite correctly—that the two kinds of rulers are logically inconsistent, with the former concerned with "making or constructing" from the "available raw materials" (p. 1207), while the latter is concerned more with *techne,* or "crafting" solutions or means to achieve an already structured set of ends (see p. 1213). The incongruency between the two types of rulers leads Steinberger to declare that Plato's claim that the "guardians and philosophers are the same person" (see *Republic* 502d–503b) is a logical paradox (p. 1223). In support Steinberger goes to great lengths to show that the philosopher and the guardian cannot be the same person, including the differences in their education, character, and relationships to the necessities of the political world, specifically the issue of deceit. As a result of this paradox he concludes that Plato's view of leadership and rule is incoherent and the addition of the philosopher-king to the *kallipolis* superfluous.

The argument made by Steinberger is interesting and well formulated—given its foundation. Much in his analysis is agreeable in the same way that Ptolemy's astronomy is agreeable: it comports quite well with the foundation as given; but if the foundation itself is deficient, predictions based on it can only be seen as coincidence or contrivance. In other words, while Ptolemy may have explained rather well a universe in which the planets revolve around the earth, he failed to explain properly *the* universe, in which the earth and the other planets revolve around the sun. While Steinberger elaborates quite well the problems with the model he describes, it is not, in fact, Plato's model.

While the *Republic* is obviously a multifaceted document that can be read on may different levels, certain parts of its structure cannot go overlooked or unaccounted for without doing harm to its meaning. The fundamental project of the *Republic* is to define justice. All the discussions of art, music, geometry, politics, leadership, cities, food, and so on are but constructs to facilitate this project. On this basis I would contend that one can view the whole of the *Republic* as revolving around three questions, namely, Can justice be found or created in the world of politics? If so, what does it look like and how is it to be achieved? If not, what does this tell us about the world of politics (if what we seek is justice)? Steinberger simply asks the wrong questions. Rather than taking Plato at his word and asking about the relationship of philosophers and guardians to the world of justice, he asks about their relationship to the world of politics. By so doing, he creates a "reality" to which his conclusions are applicable but fails properly to address the reality suggested by Plato himself.

Plato's first city (*Republic*, Book 2) is a natural city based on the "spontaneous ordering of crafts" (p. 1209), which becomes the *just* city when "tasks are performed properly" by "those whose talent and training is appropriate" (p. 1209). This city "is not something made, not the produce of artifice, it simply comes into being" (p. 1210). This being the case, though the crafts themselves are rationally ordered, the ordering itself is not the product of a craft (p. 1210); thus, "it seems that there is no ruler in this city" (p. 1210). Not only is there no ruler, but there is also no "ruling, law, or political authority in the usual sense" (p. 1210). In other words, in Plato's first city there are no manifestations of politics at all—only the cohabitation of a specific geographic area by people engaged in mutual activities of production and exchange of their own crafted goods. Politics, with its rulers, laws, and authority, does not exist in Plato's first city; it is a community but not a political community. Socrates insists that this first city is the "true city" or the "healthy city" (p. 1210). Steinberger makes his critical mistake when he claims that "this city is in some sense a model for all cities" because it offers a model of order and harmony and "provides its citizen with a life of peace, prosperity, and happiness" (p. 1210). Rather, I would argue, it is a model for *cities without politics*. The *kallipolis*—the chief model elaborated in the *Republic*—is a model for quite a different type of city.

When Glaucon calls the city Socrates has described at first as a "city of pigs" and induces him to speak on a more luxurious city, the reader must be careful to note that doing so was not Socrates' idea. Steinberger correctly notes that humans "multiply their desires far beyond what is natural and necessary" (p. 1210). But not so Socrates or the philosopher in general. The concern for luxury that the majority of people share with Glaucon inspires Socrates to explore what is called the "feverish city." Only then is it necessary to introduce politics and its manifestations. Steinberger correctly notes that "at the core of the *Republic*, then, is an effort to purge (in thought) this feverish city of its excesses, to restructure it to approximate the health and orderliness of the first city" (p. 1210); yet he is wrong to say that this means that "the virtue of the *kallipolis*, like that of the first city, lies in how well its parts fit together into an organic whole" (p. 1211). The *kallipolis* is not "organic" at all in the way the first city was. Rather, the *kallipolis* needs political (read *human*) intervention even to approximate the first city. Once Socrates has left the discussion of the first city behind, he has left behind the possibility of justice in at least the strong sense of that which is natural, spontaneous, and self-ordering (read *self-sufficient*). Instead, what we have once the shift to the luxurious city is made is an attempt to have our proverbial cake and eat it too. In other words, Socrates offers his readers a *glimpse* of the type of city where justice might flourish, namely, a simple world of craft and exchange where each person has enough to sustain life but not much more. In this world Socrates or any other philosopher could be happy and lead the philosophic life unencumbered by either political restrictions on what he might think and say or worldly or political duties such as ruling. But this isn't enough for Glaucon or most other people. They want all this and luxury too. The task of Socrates for the rest of the dialogue then becomes to create a community where luxuries and the politics that restrain the abuses of such things do not prevent the practice of philosophy or the discovery of justice. The question that then haunts the *Republic* is whether this state of affairs is possible—hence the question of ruling.

In the first city there are no rulers. Each person rules himself or herself because politics and its manifestations (rulers, laws, and authority) are nonexistent. Only when luxury and its excesses come to mark the city does politics in the form of restructuring for health and order become necessary. The artificial city, which lacks the spontaneous order and harmony of the first city, is the one that needs politics, or ruling. Ruling, or what Steinberger calls "managerial roles," is "necessitated by the fact—or persistent threat—of *disease* or *fever*" (p. 1211). These roles can be filled by the guardians ruling in a "craftlike sense," who might benefit by also being philosophers but do not necessarily need to be (p. 1212). Steinberger argues that the model with the guardians is complete even before the introduction of the philosopher-king (p. 1215), but here his failure to keep the fundamental project of the *Republic* in mind betrays his analysis. The *political* model is in fact complete without the philosopher-king, for the guardians are able through *techne* to maintain order in the city. But Socrates and his companions set out to find not a political model but the nature of justice; and for this the rule of the guardians is not sufficient.

Steinberger admits a role for the philosopher-king in reforming existing cities but aside from this argues that their introduction into the *kallipolis* adds "little to the integrity of the model" (p. 1216). True, the philosopher-king adds very little to the political model, or the *kallipolis,* because the guardians are able to maintain order or contain the fever by themselves. But the just city and the *kallipolis* are not fundamental equivalents but rather two different kinds of communities—one artificial and one natural. The justification for bringing the philosopher-king back into the *kallipolis* is to reintroduce the original task of the dialogue, namely, finding justice. The political world, or the world of luxury suggested by Glaucon, has been explored and found wanting by Socrates. The guardians can rule over the political approximation of the just city but they cannot make it just. The role of the philosopher-king, as we see in Book 7, is to *destroy* the *kallipolis* by getting the people to "to despise the current honors" and by killing off all those over the age of ten who desire (as Glaucon desires) the self-contradiction of a community that is both luxurious and just. Justice, the original goal, cannot be found in the political world (though many would like to believe it can.) Plato does not "fail to revise the institutional structure of the *kallipolis*" (p. 1222). *He* never authorized its erection in the first place. He merely demonstrated why Glaucon's request was unreasonable, given the object of their search.

The *best* the political world can offer is order and harmony, but order and harmony are not justice. Though still undefined at the conclusion of the dialogue justice must clearly be something "natural and spontaneous," the product not of human artifice but of human contemplation and discovery. Justice is not to be found in the political city, which becomes necessary only when human desires in the physical world exceed what is necessary for their existence. The city of Glaucon, or the *kallipolis,* represents people's wish to have all their desires met and still have justice. Socrates elaborates all of the features that such a city will need to survive; and if most of those rather harsh and oppressive solutions seem undesirable, it is to make us see that the journey from the city of "necessity" to the city of "luxury" isn't worth the trip. Only in the political world do men and women need guardians to protect them from each other and from themselves; the rule of the philosopher-kings makes the apolitical city possible again. When they rule, there are no more "guardians" in the political sense, for *the two have become one.*

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I am grateful to Christopher M. Duncan for his effort to consider and assess my views on Plato. It is a pleasure to have one's work taken seriously. Unfortunately, I do not see that his comments have any force against my argument.

Indeed, it is not entirely clear in what sense his criticisms are criticisms at all, since he explicitly accepts my distinction between guardians and philosopher-kings, my characterization of the features of the city of pigs, my account of this city as the true or healthy city, my interpretation of Socrates' claims concerning luxury and unnatural desire, my description of the central aims of the *Republic*, my interpretation of the necessity and purpose of ruling in the *kallipolis* and the craftlike quality of the guardians' rule, and my contentions that Plato's "political model" is completed prior to the introduction of the concept of the philosopher-king and that philosopher-kings are not part of the *kallipolis*. He thus concedes nearly all of the important claims that my essay makes.

His main argument seems to be that whereas the first city (the city of pigs) and the third city (the city of philosopher-kings) are fundamentally similar in being nonpolitical embodiments of justice, the second city (the *kallipolis*), characterized by the presence of rulers, is political and not at all just. My error apparently is in failing to see this, hence failing to understand that the *kallipolis* is a political model but not a justice model. This argument in no way confutes, but is fully consistent with, the central contention of my essay that there is a deep difference between guardians and philosopher-kings. As far as I can tell, then, Duncan is criticizing only one relatively minor corollary of this contention—namely, that there is an internal contradiction in the *Republic*. One can understand why such a claim would attract attention. But in fact, I would be perfectly willing to reject this corollary with equanimity, since doing so would have no serious consequences for the central argument of my essay.

Unfortunately, Duncan's formation does not work even against the corollary because it attributes views to Plato that he cannot possibly have held. Duncan's argument rests entirely on his distinction between *justice models* (the city of pigs and the city of philosopher-kings) and *political models* (the *kallipolis*). But there is, in fact, no textual, conceptual, or theoretical basis for such a distinction; hence, the argument derived from it cannot be correct.

First of all, the notion of *the political* is used by Duncan in a highly non-Platonic manner, since for Plato the political would presumably refer to all things pertaining to the polis. Thus, for example, the phrase *apolitical city*, which Duncan predicates of his justice models, makes no Platonic sense.

Second, one might attribute this error simply to an imprecise use of language and say that by *apolitical* Duncan is really referring to the absence of an explicit ruler or group of rulers. There certainly are no rulers in the city of pigs. But Duncan claims that this is also the case with the city of philosopher-kings, a most implausible claim. Whereas the city of pigs arises and functions "naturally and spontaneously" (i.e., without human artifice or rulers), this can hardly be true of the third city. Philosopher-kings are kings. They do not gravitate spontaneously to the cave but are compelled to return there; once there, they presumably propagate laws and policies explicitly and intentionally designed to ensure that empirical cities function as harmoniously as possible. There is no unseen hand here. The city of philosopher-kings is not a spontaneous, unplanned organism like the city of pigs; it is indeed the product of human artifice and rule, albeit guided quite strictly by the dictates of right reason.

Third, Duncan's claim that the *kallipo-*

lis is in no sense a matter of justice despite the fact that it is well ordered and harmonious is equally implausible. He argues that *justice* remains "undefined at the conclusion of the dialogue." I do not know how to reconcile this with the famous passages in Book 4 (e.g., 427b–435a) where Plato certainly seems to provide an explicit and powerful definition of *justice.* Justice is, indeed, a matter of correct order and harmony, whether of the city or of the soul. To the extent that such order and harmony is characteristic of the *kallipolis,* it must be like the city of pigs and the city of philosopher-kings in being a representation of justice. The distinction between *political city* and *just city* thus has no textual basis.

It may be that Duncan's misinterpretation is rooted in a series of erroneous readings, both of Plato and of my essay. He begins by citing a distinction between ruling as "making or constructing" on the one hand and "crafting solutions" on the other. In fact, there is no such distinction. The distinction I drew was between ruling as *techne* (which includes making and constructing, as well as crafting) and ruling as philosophy. He discusses the happy and unencumbered life of philosophy in the city of pigs, whereas Plato's account of that city in fact makes no mention of philosophy. He accuses Plato of advocating genocide—of wanting to murder most individuals over the age of ten—when in fact Plato wants only to exile them to the countryside. The latter may be a fate worse than death, but it's hardly the same thing.

For all these reasons, I do not find that Duncan makes a plausible case even against the relatively minor corollary that his comment tacitly addresses. This somewhat surprises me, for I had expected his criticism, along with comments from various other interlocutors, to be far more telling. My purpose in writing the essay was to provide an account of the *Republic* that was both original and accurate. Given the enormous amount of extraordinary philosophical talent that has been devoted to interpreting Plato over many centuries, I assumed at the outset that achieving such a goal would be highly unlikely. As a purely probabilistic matter, any original account would almost certainly be wrong and any correct account would almost certainly be unoriginal. This is still my view. I presume that my essay must be either old hat or in error, and I am thus waiting only to learn in exactly what ways this is so. Duncan has not been helpful in this regard, and this leaves me slightly—though only slightly—less confident that my argument is somehow defective.

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Forthcoming in March

The following articles, research notes, and controversies, have been tentatively scheduled for publication in the March 1991 issue:

Paul R. Abramson and Charles W. Ostrom. "Macropartisanship: An Empirical Reassessment." A research note.

David P. Baron. "A Spatial Bargaining Theory of Government Formation in Parliamentary Systems."

James W. Booth. "The New Household Economy."

Jeffrey Cohen, Michael A. Frassa, and John Hammond. "The Impact of Presidential Campaigning on Midterm Senate Elections." A research note.

Lee Epstein and C. K. Rowland, "Debunking the Myth of Interest Group Invincibility in the Courts." A research note.

Joshua Goldstein and John R. Freeman. "U.S.-Soviet-Chinese Relations: Routine, Reciprocity, or Relational Expectations?"

B. Honig. "Declarations of Independence: Arendt and Derrida on the Problem of Founding a Republic."

Timothy Mitchell. "The Limits of the State: Beyond Statist Approaches and Their Critics."

Ronald B. Rapoport and Alan I. Abramowitz. "Do Endorsements Matter? Group Influence in the 1984 Democratic Congress." A research note.

John T. Scholz. "Cooperative Regulatory Enforcement and the Politics of Administrative Effectiveness."

Cheryl Schonhardt-Bailey. "The Best of Both Worlds: Industry Concentration, Export Sector Deconcentration, and the Emergence of Britain's Nineteenth-Century Free Trade Lobby."

Judith Shklar. "Redeeming American Political Theory."

Richard C. Sinopoli and Nancy J. Hirschmann. "Feminist and Liberal Theory." A controversy.

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READING, WRITING, AND ARITHMETIC: DIVIDING THE DISCIPLINE

The Limits of Political Science. By Nevil Johnson. Oxford: Oxford University Press, 1989. 142p. $35.00.

This book will be to the liking of those in hot pursuit of what is best described as "après-postbehavioralism" or any other "après" that follows some "post" (say, poststructuralism or postfunctionalism). It was evidently written for the benefit of those who did not have the good fortune, as I had, to do their graduate education in political science in the 1920s or 1930s, before our undisciplined discipline was polluted by what the author calls "politics as present action" and "politics as science." I cannot possibly describe, explain, and controvert here what one might call the author's "Oakeshottish" vision of politics. I can only suggest taking a look at this little book partly for amusement and partly for enlightenment—to discover that wisdom and knowledge are not the same thing; for the author says some wise things about political science, especially as to "the illusions of utility" cherished by some but also equally many—and in fact more silly and ill-informed—things about what, thirty years ago, another English author had called "the *American* science of politics."

It is probably a mistake to review *The Limits of Political Science* in the context of political science as an advancing discipline that seeks to extend any frontiers of theory and research. Rather, its frame of reference is a view of undergraduate education in British universities (notably Oxford) that is essentially humanistic and is evidently threatened by what is going on at a place like Essex (an American colony?). I need quote only two passages to convey what the book is about and its tone: "Politics as a branch of study has today become shapeless and somewhat negligent. It has been corrupted by the fascination of the passing show, and many have yielded to such temptation. It has been made esoteric by the pursuit of an inappropriate scientific model, a possibility which some have espoused with enthusiasm" (p. 136); and (therefore) "for politics as a general education, a training of the mind, it is necessary to identify the subject again as belonging firmly to what we can designate according to taste as the moral sciences or the humanities. It is for this reason that it must be seen as emanating from history and requiring the challenge of philosophical analysis" (p. 136).

Why it is necessary to misrepresent modern (post-Oakeshott?) political science, especially as it has evolved in the United States, in order to advocate a return to a classical view, escapes my unpolemical mind (I'm trying hard, clearly). The author's knowledge of contemporary research in political science is so mistaken as to be nonsense. He does not seem to have the vaguest idea of what, say, U.S. scholars as diverse as Almond, Apter, Berger, Burns, Converse, Dahl, Deutsch, Eckstein, Eldersveld, Epstein, Fenno, Ferejohn, Huntington, Jones, Lane, LaPalombara, Pitkin, Miller, Organski, Polsby, Ranney, Riker, Rosenau, Salisbury, Shklar, Singer, Tarrow, D. F. Thompson, Wahlke, Waltz, Wildavsky, Wilson, Wolfinger, and Zinnes are all about. But then, you will find no reference to them in the book that nevertheless, presumes such knowledge. As for the work of social philosophers like Nozick or Rawls—"There can be little doubt about the narrowness of the social experience on which it rests and the naiveté of many of its underlying moral and social assumptions. As a result it is all too easy for attention to be confined to a limited range of questions that tend to reduce political theory to a form of parochial moral casuistry" (p. 109). I leave it to the reader's imagination to figure out what the author has to say about "short-sighted positivism" (p. 107); "the drily empirical idiom of politics," or "naive empiricism" (p. 108–9); and other enemies of Oxford-style political education.

The funny thing about this book is that it is fun to read because it is so eminently wrong-headed yet has some interesting things to say about some disciplinary fads and foibles. The author strikes me as a kind of contemporary political-scientist-by-osmosis who, a hundred years ago, would have been more comfortable in John W. Burgess's seminar room at Columbia or Woodrow Wilson's lecture room at Princeton than he seems to be at Oxford today.

HEINZ EULAU

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Craftways: On the Organization of Scholarly Work. By Aaron Wildavsky. New Brunswick: Transaction, 1989. 155p. $29.95.

Herbert Muller once said, in the context of stressing the inevitably personal aspect of every writer's work, that the true title of every book is *How To Be More Like Me.* His remark is especially applicable to Aaron Wildavsky's *Craftways.* This is not an introduction to methodology—at least not in the ordinary sense of the term. There are no arcane disputes on epistemology, or chapters on values in social research. The book is concerned, as the subtitle suggests, with the immediate and concrete problems confronted by scholars in their research. The "data" on which this very personal study is based are Wildavsky's own experiences as a scholar.

Part 1, "Writing and Reading," looks at how habit, routine, and rhythm can support thinking and writing. It looks at the problems of getting the most out of limited time and energy and balancing the demands of scholarship against those of teaching and administration. "Rationality in Writing" and "Reading with a Purpose" take up the problems of putting what you know into a pattern, what to do when the pattern refuses to materialize, and how to make the best use of the time that you can devote to reading.

Part 2, "Research," includes "The Organization (and the Defense) of Time in Scholarly Activities" and chapters on interviewing and collaboration. The chapter on interviewing, written in collaboration with Dean Hammer, is very highly developed and detailed. Obviously the fruit of much experience as well as a knowledge of the literature on the subject, it would be useful to anyone who uses interviews in research and interesting to many who do not.

Wildavsky cites his own works again and again and constantly refers to experiences with colleagues, students, friends and acquaintances in order to illustrate and explain his ideas. This may irritate some readers, but Wildavsky is a very good observer and has many astute comments and suggestions to make on situations and predicaments familiar to anyone who has ever undertaken some serious research. His comments on ethical considerations in interviewing are especially well honed. I would recommend this book in particular for advanced graduate students and young scholars.

Part 3 is entitled "Purely Personal." The first two chapters tell of the travails of Wildavsky's family in the Ukraine, their immigration to America in the aftermath of the Bolshevik Revolution, and their arduous but successful struggles to carve out a place for themselves in New York. Returning to the introduction, I found that these chapters were intended to teach something about the love of *craftsmanship* subliminally by showing examples of it in members of his family. I love stories about coming to America, so I didn't mind reading these. But if they added any insight to what I had already learned about craftsmanship, it must still be at a subliminal level. The last chapter, "Things I Never Knew," is devoted to warm tributes to former teachers and colleagues.

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The Elusive Quest: Theory and International Politics. By Yale H. Ferguson and Richard W. Mansbach. Columbia: University of South Carolina Press, 1988. 312p. $29.95.

Inquiry, Logic, and International Politics. By Benjamin A. Most and Harvey Starr. Columbia: University of South Carolina Press, 1989. 243p. $39.95.

Formal Theories in International Relations. By Michael Nicholson. New York: Cambridge University Press, 1989. 254p. $49.50.

These three books share a common issue: the viability of a scientific approach to the study of international relations (i.e., the extent to which the subfield that Singer [1968] originally dubbed "quantitative international politics" has been successful). They provide, however, strikingly different positions. *The Elusive Quest* begins by noting that "we are not merely disappointed with the slow progress that has been made in transforming the field of international relations into a scientific discipline, . . . we are increasingly uncertain whether such a transformation is possible at all" (p. 4). At the other end of the continuum, *Formal Theories in International Relations* argues that "formal and mathematical international relations theory . . . is a growing field, which, though regarded merely as a passing fashion by some, holds great promise" (p. xi). Somewhere in between, *Inquiry, Logic, and International Politics* posits that quantitative international politics (QIP) has not done as well as it could in cumulating knowledge because the "research triad—logic, theory, and methodology" (p. 9) has not been adequately blended, expressing the concern "that research design may not be logically consistent with the empirical phenomena and related theory it is supposed to study" (p. 4). But the reader, hoping to draw a conclusion through a comparison of the three arguments, will find much here that will frustrate and confuse. Unfortunately, the brief either for or against a more formal, scientific approach to international politics is not to be found in these three volumes.

Although Ferguson and Mansbach assert that progress in QIP has been slow and that they are uncertain whether a transformation of the field of international relations into a scientific discipline is possible, they supply no evidence to support these contentions. The book begins with the premise that science and the study of international relations don't mix and proceeds from there to try to trace the reasons for this oil-and-water phenomenon.

The explanations they propose vary widely. Chapter 1 suggests that part of the fault lies with the overeagerness of QIP researchers to use the Kuhnian paradigm as the appropriate model for how science should proceed in the social sciences. Focusing on paradigm shifts has made it all too easy for the typical QIPer not to build on past research and thus be oblivious to the noncumulative nature of the enterprise: "The field of international relations . . . has not developed by a process of incremental or continuous theoretical growth" (p. 13). By legitimizing "abrupt intellectual shifts" (p. 15) "the Kuhnian framework constituted a device that allowed international relations scholars to see progress in their field while surrounded by theoretical incoherence" (p. 22). The implication seems to be that QIP would have made greater theoretical progress had Kuhn never lived—a rather strange proposition.

Chapter 2 posits a second explanation, "the impossibility of freeing research from its normative roots" (p. 33). Realism and idealism are used as examples of theories that fall short because they are grounded in normative premises. Debates between proponents of one or the other, argue the authors, are debates that "reflect different normative commitments" (p. 35). However, few if any serious QIP researchers would accept either realism or idealism as a scientific theory. Showing that these "theories" do not hold water is hardly a critique against QIP. But even if one were to accept these "theories" as components of QIP, their dismissal on the grounds of their normative bases takes us back to the old arguments of the behavioral revolution some decades ago. The choice of any research question has its roots in the tastes and values of the researcher. A key virtue of the scientific enterprise, as opposed to other forms of inquiry, is that its rules limit the extent to which normative biases can enter and at the very least make it evident where and how the biases affect the study.

A third explanation is found toward the end of chapter 1. Here the authors assert that the problem lies in "the nature of the beast itself. The study of politics . . . is peculiarly unamenable to empiricism. Its concepts . . . lack objective referents, . . . they defy consensual definition" (p. 23). This lack of consensus on basic concepts prevents the identification of common problems or puzzles; so "groups of scholars . . . continue to pursue idiosyncratic lines of research, often in isolation from one another" (p. 113). Unless one considers the material in chapters 5 and 6 as an effort to support this contention, the argument hangs suspended in midair. On the other hand, if chapters 5 and 6 are interpreted as evidence, the result is less than convincing.

Chapter 5 consists of a list of meanings that the concept of the state has had: an "ethnocultural unit" (p. 129), a "normative order" (p. 130), an "entity which has a monopoly of legitimate violence" (p. 136), and so on. But multiple meanings of a concept do not make it "unamenable to empiricism," as the large number of data sets based on operational measures of the state (p. 6) illustrate. Chapter 6 provides an analysis of a second concept: rationality. The authors believe that this concept is fundamental to the study of foreign policy. But, they conclude, "most of us . . . have stopped fooling ourselves . . . as far as the usefulness of the rationality assumption [is concerned]. . . . Few today would accord it more than very limited utility. . . . Without it, however, little remains that might aid us to reconstruct consensual foreign policy theory" (p. 160). Unlike chapter 5, there is no attempt to demonstrate that rationality, either as concept or assumption, is "unamenable to empiricism"; nor is it clear why this concept is essential for the study of foreign policy. More generally, the reader is surprised to find that only two concepts are used to bolster the argument. Since the authors could have had even more fun with concepts like *power*, chapters 5 and 6 seem singularly incomplete.

The difficulty with this third attempt to demonstrate that QIP is not possible is that the pieces do not fit together. It is one thing to argue that concepts are not operationally measurable, another to contend that there is no consensus on concepts whether measurable or not, and yet a third position to assert that the concepts of politics "defy consensual definition." To draw conclusions about the viability of QIP requires that the first proposition be substantiated. Yet chapters 5 and 6 address the second. Although the third proposition could be used to infer the second, there are no other logical links between these three propositions. Thus chapters 5 and 6 do not provide a basis for dismissing QIP, and I, at least, am unwilling to accept the authors' conclusion that "far from cumulation, . . . the field is consistently in the grip of fads that, like novas, light up the scholarly heavens for a brief time and then disappear" (p. 23).

Chapters 3 and 4 provide a historical sketch from the Middle Ages to Machiavelli to illustrate how the norms of an age contribute to and determine its attempt to theorize and characterizations of the realist and idealist schools of thought to demonstrate that they are "less coherent bodies of theory than they are competing bundles of norms" (p. 6), respectively. Although the historical sketch in chapter 3 contains some interesting material, its bearing on the issue of science in international politics is somewhat oblique. And, as noted before, the destruction of realism or idealism as theories is largely the destruction of a straw man.

Chapters 7 and 8 examine several more recent developments in theorizing to ask whether these areas have had more success in becoming theoretical and/or scientific. Chapter 7 examines the area of comparative foreign policy following the lead of Rosenau and concludes in the negative, noting that "the main problem is that we have too many choices of approaches, a host of 'new' variables. . . . We have an expanded checklist of factors that may influence foreign policy but no sure way of assessing their importance." Chapter 8 considers "anarchy and interdependence" in their new guises as systems theory, integration theory, development theory, or regime theory (p. 8) but concludes that "it is largely old wine in new bottles and has been no more successful . . . in producing explanatory theory" (p. 8).

Thus, it comes as no surprise in the concluding chapter that "the self-styled scientific revolution in international relations was mainly methodological, . . . there was no agreement of any kind from the outset on a common research agenda. Theorists started off in any number of different directions, with scant consensus as to the basic puzzle" (p. 216).

But while readers are unlikely to be convinced that QIP is an "elusive quest," they are also unlikely, though for rather different reasons, to be ready to accept Nicholson's optimistic premise that it "holds great promise." In his *Preface* Nicholson indicates that his purpose is "to give a critical account of the present state of formal and mathematical international relations theory and evaluate its current status and hopes for the future" (p. xi). The difficulty is that the volume does none of these things. It is clearly not a review of the major contributions of formal modeling in international relations, for it cites very few modeling studies. Many of the standard studies never get mentioned, and a few of the rather more esoteric areas are covered in great detail. Catastrophe theory, for example, which has provided almost no major contributions, gets a whole chapter; while the Bueno de Mesquita models are barely mentioned. The volume is also neither a critique of the contributions that formal models have made to the field nor an evaluation of the current status of formal models in international relations. It is not, in short, a rebuttal to the elusive quest, as its author apparently intended it to be.

Formal Theories can best be described as a collection of mathematical ideas that have been, or might be, useful for understanding research questions in international relations. Following an attempt in the first two chapters to delineate the domain of international relations questions, provide a brief discussion of the introduction of mathematics to the social sciences, and sketch some underlying "presuppositions of a formal approach to international relations" (including an important distinction between mathematical models on the one hand and mathematical pictures on the other), the book takes off into discussions of a variety of mathematical concepts. Nicholson argues that the origins of formal modeling in international relations can be traced to the works of Richardson and Von Neumann and Morgenstern. Not surprisingly, then, the volume focuses on the mathematics of dynamic systems and game theory.

Chapter 3 defines a prisoner's dilemma and related concepts (supergame, metastrategies) and proceeds to show how the structure of the game (and consequently the behavior of the players) can be altered by introducing new assumptions. The noncooperative behavior that inevitably occurs in the prisoner's dilemma can be changed into cooperative behavior by introducing multiple plays and a discount parameter that captures the social context by making the future less important than the present. Nicholson goes on to show that the discount parameter can be interpreted as a measure of uncertainty. Having defined these basic concepts, Nicholson then illustrates their usefulness through a game-theoretic interpretation of the Suez crisis.

Chapter 4 sketches basic concepts and ideas of probability theory: axioms of probability theory, conditional probability, Bayes' Theorem, philosophical issues concerning subjective and objective probability, axioms of utility, and means for combining multiple probabilities. The chapter concludes with a brief discussion of how information theory might be used to measure the amount of uncertainty. Chapter 5, like the conclusion of chapter 3, provides a series of international relations examples that illustrate how the probability concepts of chapter 4 might be used to think about nuclear war, deterrence theory, and the verification of arms agreements.

Chapters 6 and 7 extend the game-theoretic presentation by describing the bargaining process in the context of a game and the process of coalition formation in the context of n-person non-zero-sum games. Basic solution concepts (e.g., Nash) are defined and illustrated, and the problem of collective goods is discussed in its relationship to coalition theory. Unlike chapter 3 and 5, however, these chapters' international relations illustrations are minimal and largely confined to a presentation of a model developed by Olson and Zechauser to explain burden sharing in alliances.

Chapter 8 moves away from game theory and begins laying the groundwork for the use of dynamic systems. The Richardson model is presented, first in a static form to illustrate some of the basic concepts and then in its original dynamic form. Nicholson notes that the Richardson model can be derived from a set of other assumptions, using a model developed by Abelson some years ago and then goes on to show how the original two-nation model can be extended to n nations. Although the focus is on the Richardson model, the chapter's mission is clearly to provide an understanding of basic concepts, not a review of the Richardson model literature. None of the quantitative work that has been done on the Richardson model, nor are any of its more elaborate subsequent reformulations discussed.

Presumably because the Richardson model is linear, chapter 9 considers nonlinear dynamical systems, more particularly catastrophe theory. Again, basic concepts are provided, and some attempt is made to use these ideas in the context of arms races. The nonlinear theme continues into chapter 10, where applications of catastrophe theory to the study of war are considered and the outlines of a nonlinear systems-level model introduced by Zinnes and Muncaster is described. The chapter strangely concludes with a discussion of graph theory—which is neither dynamic nor "nonlinear"—and its usefulness for the study of polarity issues in international politics. Presumably, its inclusion is a function of its usefulness for the study of system-level properties, another theme that runs through the chapter.

In the final substantive chapter, Chapter 11, we move back to game theory and nonsystemic levels of analysis. The concept of satisficing and its relevance to hypergames is explored. But like chapter 10, this chapter confusingly concludes with a discussion of cognitive maps that looks more appropriate to the presentation of graph theory in the previous chapter than it does to hypergames.

Thus, *Formal Theories* provides the reader with a set of mathematical ideas (or mathematical "tools," as the author notes in one place), that are potentially useful for understanding international relations. It is not a volume that will persuade the unwashed. Nor will it be required reading for the convinced. The mathematical tools are fairly fundamental and thus relatively well known among mathematical modelers. Its audience therefore lies somewhere in between.

The advanced graduate student who is sympathetic to the modeling approach but lacks the mathematical skills to benefit from the modeling literature could well use sections of the volume as background information. Each of the mathematical presentations is well presented and lucidly written. Moving the reader through a static Richardson model and then exploring its dynamic counterpart is a nice way to ease the uninitiated into the enterprise. A number of the game theory sections are exceptionally well developed. The material, however, would have to be augmented in at least two ways.

First, a student would need to be shown the broader picture, the framework within which the mathematical ideas fall. The book presents the mathematical concepts in an almost helter-skelter fashion. Transitions between chapters are nonexistent—why, for example, do we have several chapters on game theory, then a series of chapters on systems analysis, and then another chapter on game theory? What is the relationship between the Richardson model and catastrophe theory? Although I have tried to suggest a logic that might run from chapter to chapter, this is my interpretation: none is provided by the book. This feeling of scatter is also evident within chapters (e.g., in regard to graph theory).

Second, the student would need to have the book's material integrated into the research literature. Why are *these* mathematical ideas important, who has used them, and to what end? The work of Hart or Harary should be seen in the context of graph theory. Bueno de Mesquita's models, both theoretically and empirically represent important examples of the expected utility concepts, Cioffi's probability models of deterrence provide useful examples of some of the probability concepts in action, the game-theoretic work by Brams and others illustrates game theory applications as well as extensions, and the extensive empirical and theoretical work that was subsequently spawned by the Richardson model demonstrates the richness and significance of his original ideas and the mathematics that underlies it. The absence of such links to the literature enhances the feeling that the mathematical tools are something of a random scatter of ideas.

Inquiry, Logic, and International Politics shares Ferguson's and Mansbach's concern that QIP has not been sufficiently cumulative but at the same time projects the more positive attitude for the future of QIP that is evident in Nicholson. Most and Starr believe that the origin of the cumulation problem lies in inappropriate research designs and incorrect theorizing. Thus, the purpose of the book is not to dismiss QIP but to point to errors that might be corrected in the future.

Following a lengthy discussion of how method, theory, and logic must interact to provide cumulative knowledge in QIP, chapter 3 discusses the first of two major points made concerning the inadequacy of many research designs in QIP. The authors argue that QIP researchers tend to state propositions that are necessary or sufficient or both but design tests that will not allow them to draw conclusions relevant to the hypothesis. If the hypothesis is stated as a sufficient condition (if X then Y), the appropriate research design must consider cases in which both X and not-X occurs. If the hypothesis is stated as a necessary condition (given Y, there must have been X) it is important to include cases in which it was possible for both Y and not-Y to occur. The authors provide some literature examples to illustrate the fact that incorrect sampling on either the independent or dependent variables has indeed occurred in the literature.

Chapter 4 adds a second point to the research design discussion: choosing the appropriate level of analysis for a given problem. The principal example is the study of war. The authors survey the research on war and find general agreement that war is at least a dyadic phenomenon. This suggests that attempts to determine the causes of war must consider the interaction between nations. But, the authors note, in some studies the level of analysis has been the single nation-state—searches for national attributes that might make nations more war-prone. Not surprisingly, the authors point out, the search for the missing attribute has been unsuccessful.

Chapter 5 considers the problem of inadequate and inappropriate theorizing. The argument is two-pronged. The reader is admonished to take care in formulating hypotheses, considering both the possibility of "policy substitution" and "alternative triggers." Although the language here is at times confusing, the points being made are reasonably straightforward. In mentioning "policy substitution," the authors appear to be calling for more imaginative ways to conceptualize, and probably measure, variables. If we are trying to understand the interaction between states in terms of a Richardson model, we should not consider the variables as simply *arms levels.* Such an operationalization may well miss the interaction because arms levels are part of a broad class of foreign policy phenomena that go under the more general rubric of *hostile behavior.* Sometimes that behavior manifests itself in the form of armament spending; but at other times it is reflected in such things as the withdrawal of ambassadors or the setting of tariffs. If a variable is too narrowly defined, we will incorrectly dismiss theoretical linkages. In referring to "alternative triggers," the authors are describing the need to consider conditional hypotheses, that is, hypotheses that hold only under given conditions. Thus, the statement *If X, then Y* may only hold given the presence of *A*. If these limiting conditions are not considered, sometimes-true hypotheses will be incorrectly rejected.

The observations of chapters 3, 4, and 5 are useful. However, it is not clear how they can be held responsible for the lack of cumulation. Rather, they would appear to point to difficulties in the research literature that invalidate known results. In addition, the language in chapter 5 is confusing. For example, policy substitution is at one point defined by saying, "Through time and across space similar factors could plausibly be expected to trigger different foreign policy acts" (p. 98). In the context of the if-then hypotheses that the authors have been considering, this sounds as if they are proposing that the same *X* can produce different *Y*s, whereas they really want to argue that *Y* needs to be broadly defined. Even more confusing is the characterization of alternative triggers: "Different processes could plausibly be expected to lead to similar results" (p. 98). This sounds as if different *X*s can produce the same *Y*, which would mean that the relationship cannot be defined in mathematically functional terms and that typical statistical analyses are not possible. The confusion over policy substitution and triggers is enhanced when the authors call alternative triggers the logical opposite of policy substitution.

Throughout chapters 1, 3, 4, and 5 the authors illustrate their points using a hypothesis that links a nation-state's *willingness* and *opportunity* to the occurrence of war. Chapter 2 sets the stage for these subsequent analyses through a construction of the argument that underlies the opportunity-willingness hypothesis. Chapter 6 is an interesting but seemingly tangential set of ideas concerning the interaction between five variables: (1) the size of international systems, (2) polarity, (3) types of dyads (e.g., major-major, major-minor, minor-minor), (4) the opportunity and willingness variables, and (5) the probability of war. The issue of necessity and sufficiency is raised, but the focus is less on this theme than it is on the relationship between the different variables.

Although the volume as a whole, makes an attempt to integrate the various ideas across the chapters, the result is not entirely successful. Similarly with the two other volumes, there is a feeling of discontinuity from idea to idea and from chapter to chapter. It is not clear, at least to me, that the volume goes much beyond the separate, previously published articles that constitute its chapters. Seen from another perspective, however—as a tribute to Ben Most, who died unexpectedly and prematurely—the publication of the volume leaves an important mark.

Is QIP viable, cumulative, moving in a positive direction? Each volume contributes insightful comments that bear on this question. None, however, is likely to alter whatever bias the reader brings to the question. And perhaps that is just as well. A good argument could be made for the proposition that the question is premature.

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Unifying Political Methodology: The Likelihood Theory of Statistical Inference. By Gary King. Cambridge: Cambridge University Press, 1989. 286p. $49.50 cloth, $16.95 paper.

Gary King's *Unifying Political Methodology* is at once an introduction to the likelihood theory of statistical inference and an evangelist's call for us to change our ways of doing political methodology. One need not accept the altar call to benefit enormously from the book, but the intellectual debate over the call for reformation is likely to be the enduring contribution of the work.

King's central thesis is straightforward: political science has borrowed techniques from many disciplines and as a result has no coherent perspective on statistical research. He presents the likelihood theory of statistical inference as an alternative perspective that at once encompasses our existing methods and that offers a coherent approach for future development of an original field of political methodology.

The noncontroversial part of the book is that the likelihood approach offers significant advantages as an approach to statistical problems and there are abundant examples of those advantages. The controversial side is whether we should embrace the likelihood approach to the extent of changing our teaching and research to focus on likelihood models rather than the usual least squares econometrics approach that is now the norm.

There is no doubt that statistical models based on likelihood techniques are coming to play an increasingly important role in political science. The widespread use of logit and probit models is simply one example of this growing acceptance. Yet until now there has not been a good text for teaching (or learning about) likelihood models. The method of maximum likelihood is not new, of course; but the existing texts are relatively inaccessible to political scientists. King's book does an excellent job of filling this gap.

In part 1, King presents the core ideas of the likelihood theory of inference. While there are more probability distributions than political scientists are used to seeing, the presentation is quite readable and does not demand more than is reasonable. By the end of part 1, readers should have a clear understanding of how the likelihood model works.

In part 1, King walks a delicate line between clear exposition and technical detail. While I think he generally succeeds, the disappointment is that it is at times not technical enough. The most glaring example is the section on properties of maximum likelihood estimators. In less than six pages King covers regularity conditions and finite and asymptotic properties. While the essential points are covered, there is not enough detail on how such properties are actually assessed. Granted that an extended discussion of theoretical statistics would detract from the goal of accessibility; but more advanced readers will find their taste whetted but not satisfied.

This quibble aside, the opening theory section is a generally excellent introduction that takes the student to a point of considerable familiarity and comfort with likelihood methods. This sets the stage for part 2, a series of applications. Chapters on discrete regression models, tabular data, time series, multiple equation models, and models with nonrandom selection show how maximum likelihood estimation can be applied to these specific problems. This is a very good extension of the theoretical chapters because it lets the reader see how the method works in actual applications. One of the strong points of the book is King's use of examples that examine real political science data using these estimation techniques. The results often show the power of these methods to extract more information from the data than most conventional analyses. Ultimately, the most convincing reason for substantive researchers to adopt the likelihood approach is its ability to fashion models that let us learn more about the substantive problem than we could before.

The shortcoming of these chapters is that readers who are inspored to use these techniques will not find any help in how actually to coax computer programs into action. Programing maximum likelihood routines is still a bit more complex than running regressions in SPSS, and enthusiasm can outpace good judgment. Perhaps an enterprising person will develop a companion volume showing how to implement each of King's applications in GAUSS. While the software barrier is significant, it is not as formidable as it might seem; and some sample programs would help convince readers that the benefits will repay the investment in learning GAUSS. (These routines can be implemented in a variety of languages, of course; but GAUSS is the most convenient and powerful, and it runs on a desktop machine.)

None of these minor flaws should obscure the fact that this is a superb book for learning about likelihood methods and their applications. King balances theory with application and keeps his audience clearly in mind. The book could be read by students who have had only one course in probability theory and statistical inference. However, the applications in the second half will be more readily understood if the reader is already familiar with regression and time series models. The book would be perfect as the text for a third course in political methodology. A more ambitious and daring curriculum might make it the focus of a core second course—which brings me to the more controversial point of the book: should the likelihood theory of inference become the standard model for political methodology, and should we revise our curricula to accommodate this?

King's core argument for the adoption of the likelihood approach is that it is intellectually coherent and it provides a unified approach to all sorts of estimation problems. This is a compelling argument. Under current practice students are indoctrinated in least squares estimation. When problems that require maximum likelihood (ML) approaches arise, such as probit problems, we teach students the ML technique as an exception to the normal least squares approach. This retards our ability to use the flexibility of ML methods. If ML techniques were emphasized from the beginning, the coherence of our approaches to statistical modeling would be significantly greater.

Maximum likelihood techniques are also quite flexible, allowing us to modify models easily to extract more information from the data. Such modifications are much less transparent using the traditional least squares models. This added flexibility is another significant advantage of the likelihood approach.

There are some reservations about the wholesale adoption of likelihood methods however. The most troubling issue is the specification of the likelihood function, which depends on the distribution of the variables (usually conditional on some parameters of interest). But, these distributional assumptions may be controversial, and the properties of the estimators depend on the assumption that the likelihood function is properly specified. King points out that in many cases, ML estimation appears robust under modest misspecification of the likelihood function, which is some comfort. It is also worth noting that we make distributional assumptions routinely when we report *t*-tests or *F*-statistics. Our current methods are not without distributional assumptions, it is simply that we accept them without thought. By forcing us to consider distributional assumption explicitly, likelihood methods are a nudge in the right direction. Nonetheless, the critique of the a priori distributional assumptions of ML is an important caution against the unthinking adoption of ML methods.

There is, of course, no requirement that we abandon traditional practice in order to benefit from likelihood methods. They can become just one more wrench in our tool kit, to be used when needed. King's book is an admirable description of that wrench. However, the intellectual debate that King's book should inspire is whether the advantages of coherence, flexibility, and power are sufficiently compelling that we should seriously consider changing our standard approach to statistical inference. Substantive work at the cutting edge increasingly requires specialized statistical techniques that are far easier to develop using the likelihood approach. In that sense, adopting the likelihood approach will make our work easier, not harder. However, the shift to likelihood methods will not come easily and will ultimately be driven by their power to address substantive problems, for which King presents a compelling case. The greater coherence and flexibility of the methods may turn out to be just a welcome side benefit.

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GORBACHEV'S USSR: AT HOME AND ABROAD

Beyond Perestroika: The Future of Gorbachev's USSR. By Ernest Mandel. New York: Routledge, 1989. 230p. $60.00 cloth, $18.95 paper.

Gobachev's Struggle for Economic Reform. By Anders Aslund. Ithaca: Cornell University Press, 1989. 224p. $38.50 cloth, $12.95 paper.

These two books contribute to the intense debate about the meaning and future status of Mikhail Gorbachev's program of perestroika and glasnost. Here, however, their similarities end. Ernest Mandel, an admirer of Leon Trotsky, provides a Marxist analysis of Gorbachev's domestic and foreign policies, the roots of the "deepening crisis of the Soviet system," and the prospects for proletarian revolution in the USSR. *Beyond Perestroika* contains no bibliography or index, provides no sources for several tables, and often omits accepted scholarly rules of citation.

Beyond Perestroika will evoke deja vu among those who have read Trotsky's *The New Course* and *The Revolution Betrayed.* Like Trotsky, Mandel identifies the "bureaucratic stranglehold" that the Communist party apparatus exercises over Soviet society as the source of systemic crisis. This apparatus opposes revolutionary change and aspires only to protect its own power and privileges.

Yet, Mandel argues, the apparatus may have sown the seeds of its own destruction. To pursue industrialization, it created an increasingly educated, sophisticated and urbanized work force. Its successful pursuit of industrialization raised expectations among the masses for economic and political betterment. The masses now find the persistence of low living standards and political repression "intolerable" (p. ix).

This circumstance could threaten the political survival of the apparatus. According to Mandel, Mikhail Gorbachev, representing the "modernist wing" of the apparatus, devised a program of perestroika and glasnost to avoid this fate. This program seeks to placate the masses and thereby avoid challenges to the status of the apparatus. Consequently, Gorbachev's reforms entail no fundamental alteration of the system (p. 151).

Ironically, many conservatives in the United States share this Trotskyite assessment. For example, in what is quickly becoming a conservative manifesto on the Gorbachev era, the essayist "Z" writes in the *New York Times* (4 January 1990) that perestroika seeks only "to salvage what can be saved of the system by halfway house concessions to economic and human reality." For both Mandel and "Z," the Gorbachev reforms merely exacerbate the Soviet systemic crisis. However, Mandel, unlike "Z," sees a proletarian revolution as one realistic possibility emerging from the failure of perestroika. This failure "will transform working-class skepticism into virulent discontent and eventually mass action" (p. xvi).

Mandel rightly emphasizes the disillusionment many workers feel with the meager results of perestroika to date. Whether this disillusionment will promote class solidarity and proletarian revolution is another matter. For one thing, Mandel's Marxist perspective largely ignores the explosive growth of ethnicity among the Soviet people, including proletarians. Ethnicity, not class solidarity, likely will prove the greater motive force in future political behavior of both Soviet proletarians and nonproletarians.

Anders Aslund has written a very different book. It seeks to provide a "deep and comprehensive" analysis of economic reform in the USSR between 1985 and 1988. Aslund demonstrates a command of the relevant published literature in both English and Russian. Numerous interviews with Soviet economists, Communist party officials, and other professionals enrich the analysis. His conclusions about the prospects for fundamental reform in the USSR are much more optimistic than Mandel's.

Mikhail Gorbachev is the star of this book (p. 25). Aslund views Gorbachev as "one of the most radical reformers in the Soviet leadership" (p. 184). Given his definition of reform, this means that Gorbachev is committed to diminishing the role of bureaucratic coordination and increasing the role of the market in the economy. Most significantly, the radical Gorbachev seeks "to eliminate the operative role of the party in the economy" (p. 143).

Aslund sees the disastrous performance of the Soviet economy since the late 1970s as impelling these expedients. He rejects official Soviet and Central Intelligence Agency data indicating continued, albeit slower, growth in the economy through 1985. Economic growth probably ceased after 1978, he contends (p. 15). Other noted students of the Soviet economy (e.g., Alec Nove) share this assessment.

Aslund is unclear on when, precisely, Gorbachev became a radical reformer. At one point, he argues that in December 1984 Gorbachev laid out "almost the whole spectrum of his reform proposals" to a Central Committee ideological conference (p. 26). Yet when Gorbachev assumed power in March 1985, Aslund indicts the new leaders for their "highly exaggerated" assessment of how easily they could revitalize the economy (p. 68). Aslund actually offers convincing proof that Gorbachev became increasingly radicalized when, by 1987, traditional reform measures (the campaigns against alcoholism and corruption, personnel changes, better quality control) proved "spectacular" failures (p. 85). The manifest political opposition to reform also radicalized Gorbachev. Aslund persuasively demonstrates that through 1988 the radical reformers never enjoyed a majority in the politburo (pp. 23–66). He identifies Yegor Ligachev as the foremost opponent of reform in the politburo. In contrast, noted Sovietologist Jerry Hough assigns this status to Prime Minister Nikolai Ryzhkov (*Soviet Economy,* January–March 1989, p. 35). Aslund characterizes Ryzhkov as a "moderate" supporter of reform (p. 37). Available evidence suggests that Aslund may somewhat underrate Ryzhkov's opposition to reform.

Gorbachev responded to his opponents in several ways. First, he often compromised with them, thereby diluting and making inconsistent many reform measures. Second, he enhanced his own political power by expelling many of these individuals from the politburo. Third, Gorbachev sought to "subordinate" the antireformist party and state bureaucracy to (better, "balance" it with) "popular control and strengthen the role of the legislature and the president" (p. 33).

Aslund predicts that "radicalized economic reform with far reaching democratisation" is the most likely scenario for the 1990s (p. 194). Yet he is under no illusion that this is inevitable. Opposition in the politburo remains the "paramount obstacle" to radical reform (p. 186). Aslund completed his book in October 1988. Since then, the removal of Viktor Chebrikov, Vladimir Shcherbitski, and Viktor Nikonov from the politburo has considerably weakened the conservative opposition. Aslund identifies resistance from the bureaucracy, potential labor unrest, and ethnic strife as other major obstacles to radical reform. He rightly considers the latter obstacle "so great that it may determine the course of the economic reform" (p. 188).

Aslund clearly wishes success to the radical reformers. Their failure could prove the death knell to Soviet communism. In his judgment, the next generation "appears to be too disillusioned with socialism to bother about reforming it" (p. 195). Many people in the USSR agree. Mikhail Gorbachev effectively exploits this perception by portraying himself and his program as the savior, not the destroyer, of the Soviet system.

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Federalism and Nationalism: The Struggle for Republican Rights in the USSR. By Gregory Gleason. Boulder: Westview, 1990. 170p. $27.50.

This short study by Gregory Gleason of the University of New Mexico claims to offer (1) "an analysis of the original terms of the 'federal contract' in the USSR" and (2) "an interpretation of the changes that have taken place in doctrine and practice regarding the status and function of the Soviet republics throughout the Soviet period" (p. 130).

With regard to the first-named goal, one can confidently award the author high marks. Gleason's discussion of the convoluted way in which a federal system emerged in what became the USSR is, despite its dependence on the earlier work of scholars such as Walker Connor, both illuminating and stimulating. As Gleason notes, Marx, Engels, and Lenin were strongly opposed to federalism and were convinced advocates of the formation of a unitary state; indeed, they wanted and expected all of mankind eventually to come together in a single nonnational, supranational proletarian unity. For them, national distinctions represented an important but temporary obstacle on the road to this oneness.

From the beginning, however, as Gleason points out, the Soviet leadership has combined elements of utopianism with pragmatism and ample doses of political cynicism. Lenin was forced to give the national-territorial principle legal status in an attempt to win over the minority peoples of the periphery. In Gleason's words: "Lenin's support of the federal principle was essentially tactical. Lenin saw federalism as temporary" (p. 32).

The best chapter in the book is undoubtedly the second, "A Federal Solution to the National Problem," in which Gleason carefully elucidates the reasons behind Lenin's decision to allow the formation of an implied contract between the Soviet center and the borderlands. In permitting this to happen, Lenin opposed the extreme centralizing tendencies of his chief nationalities theoretician, Stalin. A reading of this second chapter can be helpful in understanding the ramifications of the nationalities question, which has so enmeshed and bedeviled the Gorbachev leadership.

Gleason's pursuit of his second self-appointed goal—"an interpretation of the changes that have taken place in doctrine and practice regarding the status and function of the Soviet republics"—is far less satisfactory in its results. The book's subtitle would lead one to expect that its author intended a magisterial study of the on-going struggle for republican rights in the USSR. But when the book's footnotes and apparatus are subtracted, one is left with a slim text of some 110 pages, hardly adequate space in which to discuss even superficially such a vast and complicated topic.

For this reason, chapters 3–7 are necessarily sketchy, disjointed, and at times unfocused. Excellent insights can be frequently encountered, for Gleason is an intelligent student of ethnic processes in the USSR; but the text as a whole is not tightly stitched together. Brief, undigested discussions of such topics as the various Soviet constitutions and the republican ministries appear in almost pell-mell fashion. Indeed, the separate chapters themselves impress one more as discrete essays than as parts of a coherent whole. It seems obvious that given the limited length of his manuscript, Gleason would have been better served if he had concentrated on one republic or one region of the Soviet Union (say, Central Asia, his area of academic specialty) rather than attempting to encompass the entire USSR.

The sketchy and truncated nature of the book's chapters renders any serious discussion of the views on federalism held by Stalin, Khrushchev, Brezhnev, and Gorbachev impossible. Gleason makes some penetrating comments, but a great deal more ought to have been said. I thought that the treatments of Stalin's and Gorbachev's views on federalism were especially hasty and superficial. And I was struck by the fact that the views of Mikhail Suslov, Brezhnev's long-serving party secretary for ideology and sometime political rival, were simply not mentioned.

Another failing of the book is its inadequate treatment of the peculiar status of the Russian Republic. Until very recently, it has lacked such normal structures as its own Communist party, MVD, trade union and Komsomol organizations, and certain republican ministries. It still has no KGB of its own. It was surely binding upon Gleason to discuss the reasons for this apparent anomaly.

One of the stated aims of Gleason's study is to provide a key to understanding Gorbachev's handling of the nationalities problem. But Gorbachev's views on this important question are simply not elucidated. My own interpretation is that in the first two years of his tenure as general secretary, Gorbachev was consciously attempting to weaken the implied political contract with the borderlands. As is well known, he went so far as to install an ethnic Russian, Gennadii Kolbin, as first secretary of the Kazakhstan Communist party. The resulting riots in Alma Ata in December 1986 were a lesson that the periphery still took the contract seriously even if the center did not. Nevertheless, despite such protests, until very recently the Gorbachev politburo and secretariat contained only one non-Slav (Eduard Shevardnadze), an obvious affront to minority national sentiment. The makeup of the newly formed presidential council presumably represents a belated attempt to make up for such inequities; this new body includes a Kirgiz, a Latvian, and an Armenian, as well as the

Georgian Shevardnadze.

Gregory Gleason has written a useful book that can be read with profit by students of the Soviet nationalities problem.

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Moscow's Third World Strategy. By Alvin Z. Rubinstein. Princeton: Princeton University Press, 1988. 311p. $29.95.

Gorbachev's Military Policy in the Third World. By Mark N. Katz. Washington Paper no. 140. New York: Praeger, 1989. $33.95 cloth, $11.95 paper.

The Limits of Soviet Power in the Developing World. Edited by Edward A. Kolodziej and Roger E. Kanet. Baltimore: Johns Hopkins University Press, 1989. 531p. $47.50 cloth.

The Soviet Union and the Strategy of Non-Alignment in the Third World. By Roy Allison. New York: Cambridge University Press, 1988. 298p. $39.95.

Since the late 1970s, Western scholars have produced countless books and articles about Soviet policy toward the Third World. Despite this vast literature, no consensus has emerged on even the most basic issues. Some scholars, such as Elizabeth Valkenier and Jerry Hough, believe the best way to discern the motivations behind Soviet policy is to scrutinize Soviet "academic" writings and official statements. Others, including Alvin Rubinstein in his latest book, argue that "Soviet writings do little to explicate the wellsprings and aims of Soviet foreign policy" and that "Moscow's actions are the truest measure of its aims" (p. ix). Even more acute are disagreements about substantive matters. Scholars who use similar analytical techniques will often arrive at very different conclusions about the nature of Soviet objectives in the Third World.

These four books will not resolve these longstanding disagreements, but at least they will enliven and enrich the debate. The authors approach the subject from different angles and provide useful insights that complement one another, without excessive overlap. The widest-ranging of the four volumes, *Moscow's Third World Strategy,* is a well-written, meticulous survey by one of the leading U.S. experts on the Soviet Union. Relying heavily on Western secondary sources, Rubinstein traces the historical roots of Soviet involvement in the Third World, going back to the experiences of the tsarist, early Bolshevik, and Stalinist regimes. He then offers a thematic discussion of what he calls the Soviet Union's New Imperial Policy—that is, the intrusive (but not indiscriminately expansionist) strategy that Nikita Khrushchev, Leonid Brezhnev, and their successors pursued in the developing world. Although the book points out both the continuities and the discontinuities between Khrushchev and his successors, Rubinstein believes that the continuities are more important and that changes in Soviet policy after the mid-1950s stemmed primarily from the growth of Soviet power projection capabilities and the opportunities afforded by Western decolonization rather than from any shift in underlying objectives.

Rubinstein's broad historical coverage and thematic approach make his book invaluable for any introductory or upper-level course on Soviet foreign policy. Although the author should have given somewhat greater weight to Soviet writings and pronouncements about the Third World (particularly the spate of revealing articles that have appeared since Mikhail Gorbachev came to power), his book stands out as one of the best on the topic. The only problem worth mentioning, in fact, is that some portions dealing with the Gorbachev period, especially in the final chapter, have already been long overtaken by events. This is not to say that the book's whole analysis of developments since 1985 is obsolete; on the contrary, the author is on solid ground in arguing that "Gorbachev seems determined to continue an ambitious, albeit less dangerous, course in the Third World" (p. 263). Rubinstein also is justified in pointing out that the withdrawal of Soviet troops from Afghanistan did not in itself portend broader Soviet retrenchment in the Third World; indeed, the huge quantities of weapons and equipment that Moscow continued to supply to the Kabul regime long after the withdrawal was completed (in February 1989) bear out Rubinstein's caution and skepticism.

Nevertheless, the larger argument that Rubinstein makes about recent developments—namely, that Gorbachev's approach to the Third World is merely a continuation of "the policy pioneered by Khrushchev and driven forward by Brezhnev" (p. 263)—is no longer convincing. True, the Soviet Union is still vying for diplomatic advantage in the Middle East and elsewhere and Soviet arms transfers to the Third World remain exceedingly high. But this hardly detracts from the far-reaching changes that Gorbachev has been implementing in Soviet policy toward the Third World even if those changes have come about more gradually and less spectacularly than in Europe. The mounting frictions between Moscow and Havana, the sharp contraction of Soviet naval activities, and Gorbachev's willingness to accept (and even welcome) the collapse of Marxist-Leninist rule in Mongolia and Nicaragua are but a few examples. The durability of these changes may be open to doubt, but the fact that they have been occurring now seems indisputable.

Furthermore, some of Rubinstein's comments were outdated even at the time he was still writing (in the summer of 1988). It is hard to understand, for example, why he would have argued that Gorbachev "is far more respectful of the military than Khrushchev was, and far less likely to be high-handed with them in his efforts to trim defense expenditures" (p. 266). Gorbachev's campaign against the Soviet military was under way long before Rubinstein completed his book. Even more curious is the author's assertion that "the overall strategic environment . . . has become far less congenial to the United States than formerly and perhaps better suited to the advancement of Soviet than of American purposes. Relatively speaking, *the Soviet Union's geostrategic, military, political, and even economic situation may be better than that of the United States*" (p. 269, emphasis mine). This claim, whatever its merits in mid-1988, can no longer be taken seriously.

Fortunately, though, these drawbacks do not mar the lucid analysis that Rubinstein provides through most of his book. The thematic and historical overview in *Moscow's Third World Strategy* will be of lasting value, and the book's uneven treatment of the Gorbachev period can be revised in a subsequent edition.

A more up-to-date assessment of Gorbachev's policies can be found in the short monograph by Mark Katz, which covers developments from 1985 through early 1989. Unlike Rubinstein, Katz detects numerous signs of fundamental change in Soviet-Third World relations. Although Katz does not overlook elements of continuity as well as discontinuity, he rightly avers that Gorbachev's preoccupation with domestic economic reform has increasingly driven Soviet policy toward the Third World in a highly unorthodox (i.e., more conciliatory) direction.

Katz's most intriguing argument flows from his distinction between two types of Soviet military objectives in the Third World: *positive* (the acquisition of bases and resources, the protection of sea lanes, etc.) and *negative* (the denial of these same benefits to the United States). Gorbachev has drastically scaled back the Soviet Union's positive goals, as evidenced by Moscow's dwindling support for Third World revolutionary movements and the decline in Soviet overseas military deployments; but at the same time, Gorbachev has vigorously used diplomatic and propaganda channels to pursue the negative aim of preventing Western inroads into the Third World and reducing the U.S. military presence. So far, this negative strategy has produced mixed results; but Katz believes that over time Gorbachev may be more successful, especially if Third World governments come to believe they no longer face a threat from the Soviet Union.

Katz is on shakier ground when he predicts that Soviet military retrenchment in the Third World will necessarily remain highly selective. He argues that although Soviet officials may acquiesce in the downfall of a few impoverished Marxist-Leninist regimes, they will still be inclined to support Communist countries like Ethiopia that are of greater strategic importance. Contrary to Katz's assertion, however, Soviet military support for Ethiopia has already begun to fall off sharply and seems likely to continue downward. Moreover, the collapse of Communist rule in Eastern Europe should inspire doubts about any predictions of the firmness of Soviet commitments to distant Third World countries.

Although Katz's essay lacks the rich historical context and depth that Rubinstein provides, it offers a more cogent discussion of the post-1985 period. To be sure, on some issues Katz's discussion is superficial or fails to break new ground; but overall he manages to present an astute summary of Soviet military policy toward the Third World during Gorbachev's first four years. Also, unlike Rubinstein, Katz tries to convey a sense of how Soviet debates about the Third World have changed since 1985, particularly as reflected in Soviet military writings.

The broad discussions that Rubinstein and Katz provide are useful background for the sixteen essays collected in *The Limits of Soviet Power in the Developing World.* Originally prepared for a conference in September 1986, the essays have been slightly updated to cover the first two to three years of the Gorbachev period. The contributors are all well-known experts on either Soviet foreign policy or Third World regional affairs. Twelve of the chapters deal with specific regions of the Third World, focusing on the countries that have had close ties with the Soviet Union. The other four chapters and the preface, written by the editors, lay out a framework for the regional discussions and derive broad conclusions and policy recommendations. Although most of the chapters cover topics that have been amply treated elsewhere, the essays overall are thoughtful and perceptive and the editors have done a commendable job. It is unfortunate, however, that no attempt was made in several chapters to take greater account of events in 1987 and 1988, when Soviet policy began to change substantially (as in Afghanistan).

The two main—and rather unsurprising—themes to emerge from the collection are (1) that the Soviet Union's ability to obtain lasting influence in most parts of the Third World has been consistently thwarted by indigenous ethnic, national, religious, and socioeconomic divisions, as well as by the diffusion of global military and economic power; and (2) that growing pressure for internal reform, coupled with Moscow's relative lack of success in the Third World, has prompted Gorbachev to seek a "strategic Thermidor" (i.e., prolonged respite) in the superpower conflict. In contrast to Rubinstein, who argues that Soviet policy in the developing world has "brought a remarkable array of successes" over the years (p. 258), Kolodziej and Kanet question whether the Soviet Union can *ever* be truly successful, given the constraints of the international system. The difference between these two lines of argument, however, is in part a question of emphasis. Rubinstein is interested in showing how the Soviet Union has successfully affected regional alignments and denied strategic gains to the United States; whereas Kolodziej and Kanet set out to highlight the limits of Soviet power—not least in the economic sphere.

This question of how to define success arises again in the illuminating book by Roy Allison, which examines the role of nonalignment and neutrality in Soviet relations with the Third World since the early 1950s. Drawing extensively on Soviet academic and legal literature and official statements, Allison argues that the Soviet Union sought to exploit Third World nonalignment to deny military influence and access to the West. Soviet success in achieving this negative objective (as Katz would call it) in the 1950s and 1960s did not bring the same benefits that the achievement of positive aims would have. But insofar as the Soviet Union still lagged behind the West in competing for influence in the Third World, "the underlying Soviet strategy in this situation of comparative disadvantage," according to Allison, was "one of military *denial* to the Western powers" (p. 2, emphasis original). By the 1970s, however, Soviet cooperation with the nonaligned countries, as Allison observes, was mitigated by disagreements over economic matters. Moreover, the expansion of Soviet military activities in the Third World in the late 1970s (a positive gain) impeded Moscow's ability to use nonalignment for negative purposes because fears of the greater Soviet presence induced a number of developing countries to compensate by aligning themselves militarily with the West.

Thus, a trade-off can arise between positive and negative military objectives. The Soviet Union's success (or lack of success) in achieving one category of objectives at a particular time will not necessarily be an indicator of its broader success (or lack). Allison, Katz, and Rubinstein have rightly underscored the importance of negative objectives in Soviet military dealings with the Third World. Their books should motivate other scholars to examine Moscow's capacity to deny important gains to the United States even when the Soviet Union itself is unable to achieve positive military goals.

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Soviet Relations with Latin America, 1959–1987. By Nicola Miller. Cambridge: Cambridge University Press, 1989. 252p. $39.50 cloth, $12.95 paper.

The USSR and Latin America: A Developing Relationship. Edited by Eusebio Mujal-Leon. Boston: Unwin Hyman, 1989. 408p. $39.95.

Soviet Aims in Central America: The Case of Nicaragua. By G. W. Sand. New York: Praeger, 1989. 126p. $34.95.

As the crisis in Central America escalated in the 1980s, attracting the attention of both policy makers and scholars, two principle interpretations of Soviet policy emerged. The first, exemplified by scholars such as Cole Blasier and Wayne Smith, held that the Soviet Union was instinctively cautious in its policy toward Latin America. Soviet initiatives were constrained by "geographical fatalism"—the view that since Latin America was in Washington's sphere of influence, the prospects for establishing socialist regimes there were dim. Cuba was sui generis—a historical accident unlikely to be repeated. In this view, the Soviets were apt to take advantage of opportunities to cause headaches for Washington if such opportunities arose, but they would not invest significant resources or pose any fundamental challenge to vital U.S. security interests in the region. Central America was simply too far from the Soviet Union's core foreign policy interests.

An alternative view, proposed by more conservative scholars such as Jiri Valenta, held that the success of the Nicaraguan revolution in 1979 erased traditional Soviet pessimism about the prospects for revolution in Latin America. Emboldened by visions of further victories, the Soviets launched an aggressive strategy to challenge the United States in its own backyard, hoping thereby to divert Washington's attention and resources away from the rest of the globe. This perception of threat reached its apogee at middecade, when the National Bipartisan Commission on Central America (the Kissinger Commission) echoed Reagan administration warnings that the Soviets intended to threaten the United States from its undefended southern border directly, thereby necessitating the withdrawal of U.S. troops from Europe.

Nicola Miller's *Soviet Relations with Latin America* makes a powerful case that Soviet policy did not shift significantly in response to the crisis in Central America. After presenting a brief but excellent history of Latin America's Communist parties and their relationship to the Communist International, Miller examines four cases of Soviet policy in Latin America: the forging of Moscow's alliance with Cuba; Moscow's refusal to provide significant economic aid to Salvador Allende's Popular Unity government in Chile; Soviet efforts to expand normal political and commercial relations with major Latin American countries (Argentina, Mexico, Brazil, and Venezuela); and Soviet policy in Central America.

In each case, she concludes, the Soviets proceeded cautiously, reactively, and with an eye toward their own economic self-interest. The chief lesson that Moscow seems to have drawn from its experience with Cuba was to avoid being drawn into any other politically difficult and economically costly entangling alliances with Latin American revolutionaires. Whether in Chile, Grenada, or Nicaragua, Soviet policy was consistent: Moscow refused to adopt leftist government as clients by making an open-ended economic commitment to them. Instead, the Soviets urged their erstwhile allies to normalize relations with the United States, remain a part of the Western international economic system, and go slow in building socialism. The strength of Miller's thesis is bolstered by her thorough research and carefully crafted argument drawing on primary sources from the Soviet Union, Latin America, the United States, and Europe. This book is an important new contribution to the literature.

G. W. Sand's brief monograph, *Soviet Aims in Central America,* contrasts with Miller's work in every respect. He gives a highly polemical rendition of the most extreme version of the view that the Soviet Union has a master plan for the subjugation of Central America. Rife with historical inaccuracies, his essay takes no account of the existing scholarly literature on Soviet foreign policy or Latin America, and is almost entirely devoid of primary source material. Instead, it is based largely on publications of the U.S. State Department's Office of Public Diplomacy—a special unit set up in 1983 to proselytize Congress and the public in support of Reagan's Central American policy.

There is no pretense of balance in this work;

Sand is issuing a clarion call warning of the impending apocalypse. His vision is conspiratorial: the Soviets are omniscient and virtually omnipotent in their drive for world domination, and Central America is the decisive battle. "If the United States should be unwilling to keep Central America in the Western World," he writes, "it stands to lose . . . World War III, which has now long been in progress" (p. 8). He dismisses Mikhail Gorbachev's New Thinking in foreign policy as nothing more than a deception designed to lull the United States into complacency. In short, this is not a serious book.

Mujal-Leon's collection of 12 essays, *The USSR and Latin America,* includes contributions by both Latin American and U.S. scholars with diverse points of view. It covers many of the same issues as Miller's book and, despite some differences of emphasis and interpretation, reaches similar conclusions. The lead essay by David Albright situates Soviet Latin American policy in the broader context of Soviet policy toward the Third World, with particular attention to policy debates among Soviet analysts. He concludes that Moscow has been more interested in expanding commercial ties with the large capitalist countries of South America than in extending aid to revolutionary regimes in Central America and the Caribbean. Elizabeth Kridl Valkenier concurs in her article on Soviet economic policy in Latin America. Aldo Vacs, focusing specifically on Soviet relations with Argentina and Brazil, notes that pragmatism and mutual economic benefit have been the guiding principles.

The implication of all these essays is that Washington policy makers have been a bit hysterical over the past decade about the Soviet menace in Central America. In an enlightening article, historian Richard L. Millet traces Washington's fears of communism in Latin America from early worries over Bolshevik Mexico in the 1920s to contemporary concerns over Marxist-Leninist Nicaragua in the 1980s.

In their essay on Soviet policy in Central America, Peter Clement and W. Raymond Duncan agree that Moscow has been cautious and pragmatic but nevertheless conclude that its aim in Nicaragua was to "cultivate a major client on the Central American mainland" (p. 278). This conclusion looks dubious in retrospect; in late 1989, the Soviet Union actually cooperated with the United States to pressure the Sandinistas to hold free elections—which they lost. Clement and Duncan err by failing to see Soviet economic and military aid to the Sandinistas in context: they never mention that it came at least partly in response to U.S. economic pressures on Nicaragua and the largest CIA proxy war since Laos.

The key Soviet relationship in Latin America is with Cuba, of course; and Mujal-Leon includes two essays on it. Jorge Dominguez provides a nuanced look at the Cuban-Soviet foreign policy partnership. "Cuba has carved out not only a margin of autonomy under Soviet hegemony but even a leadership role; it has induced the Soviets to make commitments that the Soviets alone might not have undertaken to the same extent," he writes; nevertheless, "hegemony means, above all, that Cuba accepts Soviet limits on its foreign policy" (pp. 159-60). Cuban autonomy is at its zenith in Latin America, Dominguez argues, because it is a region of low priority for Moscow but of vital interest to Havana. Carmelo Mesa-Lago and Fernando Gil summarize an immense amount of data on their exhaustive examination of Cuban economic relations with the Soviet Union, documenting Cuba's increasing dependence on both trade and aid from the Soviet Union during the late 1970s and 1980s. Unfortunately, their study was completed before the disintegration of socialism in Eastern Europe and the mounting domestic economic problems facing the Soviet Union, both of which will have profound economic consequences for Cuba. Since early 1990, Fidel Castro has repeatedly warned Cubans to prepare for unprecedented austerity because deliveries from the Eastern bloc are no longer assured.

Indeed, this is the major problem with both the Miller and Mujal-Leon volumes. Despite excellent research and analysis, both seem a bit anachronistic in the wake of the cataclysmic changes in Eastern Europe that demolished the post-World War II international order and augur the end of the Cold War. Even the Soviet relationship with Cuba, the bedrock of Moscow's Latin American policy for a quarter century, is suddenly problematic. As Mikhail Gorbachev struggles to contend with domestic economic dislocations and resurgent ethnic conflicts, Latin America is slipping farther from Soviet concerns than ever before.

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POLITICAL THEORY

Splitting the Difference: Compromise and Integrity in Ethics and Politics. By Martin Benjamin. Lawrence: University of Kansas Press, 1990. 195p. $22.50 cloth, $12.95 paper.

Martin Benjamin's important study of the hotly contested terrain where ethics, ideology, and political compromise meet reflects the author's philosophical training and considerable experience in the field of biomedical ethics. It is a particularly thoughtful contribution to the rapidly proliferating academic cottage industry of studies in ethics and every possible permutation of biomedical professional judgment in the United States. Benjamin's paradigm case study of ethics in political decision making is the abortion debate, though he draws a number of parallels to other related dilemmas in medical ethics.

The abortion conundrum is crucial because it exemplifies the seemingly irreconcilable clash of two conflicting world views and related ways of life (pp. 88–95). Prolife and prochoice advocates cannot easily compromise on the key question of whether basic organic indicators, self-awareness, or independent viability constitute fetal life because the answer is an integral part of complex, foundational sets of beliefs entailing not only female reproductive rights but also convictions about the scope and limits of human self-determination in the universe. Nonetheless, Benjamin himself believes that by firmly rooting the debate in political context, compromise can be defined (and defended) as something other than ethical betrayal in this worst-case scenario. His eminently sensible proposal for such a compromise (pp. 166–71) shortens the trimester window of choice but expands exceptions in the latter stages of pregnancy. If successful, it would validate splitting the difference as a method of resolving conflict in other disputes with a less explosive mix of ethical deadlock and political expediency.

Benjamin's presentation, while not breaking substantial new ground in moral philosophy or political thought, is an admirably detailed consideration of the literature from MacIntyre to Rawls. It is particularly strong on the tension between deontological ethics and utilitarianism on the one hand and communitarian arguments constructing moral harmony from a Kantian or Platonic perspective on the other. Notably absent is any deconstructionist concern for the transience and permeability of modes of discourse or historical interpretation associated with clashing world views. Benjamin takes advocates in the abortion debate at their word and does not explore the economic and political interests that might stand behind their public rhetoric. In the same way, he views *philosophy* traditionally as a centuries-long conversation distinct from (though not superior to) "contingent" worldviews that are straightforward reflections of "history and community."

Benjamin is most at ease with contemporary pluralist philosophers such as Rorty, Winch, Warnock, and Hampshire, who do not find multiplicity of ideologies an irreconcilable barrier to moral dialogue and common purpose. Critical distance is the necessary impersonal vantage point of both moral philosophy and common sense. It balances, but cannot supersede, commitments firmly rooted in particular life experience. Thus, "I know he's guilty, but he's my son" is for Benjamin a legitimate response to the conflicting foci of judgment that impinge on moral consciousness. "What makes compromise necessary," Benjamin argues, "is a multiplicity of conflicting, rationally irreconcilable world views and ways of life." But what makes *compromise* possible is that "we are also capable of adopting a more external perspective," which recognizes the contingency, and thus the equal legitimacy, of our own and others' moral judgment (p. 101).

Benjamin's defense of pluralism and tolerance as the precursors for compromise is not intended as a cloaked brief for the virtues of political liberalism (p. 101). Yet, though he promises to make this distinction clear in chapter 6, the effort does not quite succeed. Then, too, he does not wish to reduce his advocacy of compromise to a crude relativism in which ethical judgment is suspended. The latter effort to be clear about boundaries is rather more effective than the former, though his exclusionary rules tend to reinforce his apparent liberalism. Rejected worldviews are those assumed offensive to generally held notions of decent behavior. Here he clusters Nazis with "racists, sexists and rapists" (p. 125). Later, ways of life that clash with what Benjamin takes to be self-evidently foundational values in Western culture are added to the list of unacceptable proposals. Notably unacceptable are Muslim demands for sanctions against the author and publishers of *Satanic Verses*. Benjamin criticizes former President Carter for urging that a more empathetic dialogue be undertaken with Muslim leaders in order to reach a compromise on the handling of Rushdie's situation. No surrender on the principle of free speech can ever be acceptable to the moral integrity of the Western worldview, Benjamin asserts. But this reads more like an "impersonal" statement of principle that may or not be workable in particular contexts.

Elsewhere, Benjamin very effectively critiques external abstract perspectives that draw the individual out for a broader look at the littered landscape of worldviews but can do little more than provide the rules of play—including respect for one's opponent—that must structure compromise. Philosophical ethics—presumably including the pluralist or liberal canon of rights—don't help one make judgments in irreconcilable moral disputes because they don't go "all the way down" to the real context of life experience (chap. 4). The traditions of moral philosophy that he typologizes as "Kantian and utilitarian" provide procedural guidance and a framework for empathy but do not help in the dirty work of substantive compromise, which is the terrain of political practitioners working under pressure of calendars and constituencies. Indeed, Benjamin's views should make a great deal of sense to an audience of U.S. political scientists and politicians. More likely, however, he is speaking to colleagues in departments of philosophy who remain unconcerned about their increasing marginality in contemporary ethical controversies. Benjamin in turn, however, needs to be more explicit about his own criteria for exclusion of compromise when descending into the cave of political pluralism.

Chapter 6, "Compromise and Integrity in Politics," is, ironically, the least credible. It is difficult to believe that after the Reagan decade federal and state courts, together with Congress and state legislatures, will neatly split the difference along the lines suggested by Benjamin. "Political government," as he somewhat awkwardly terms the process, is quite possibly bent not on compromise but on serving up victories to one side or the other depending on the shifting political configurations. Oddly, Benjamin is uneasy with the *Roe* decision precisely because it was the outcome of negotiation and compromise among the justices (p. 165). This potent example of integrity-preserving accommodation is too full of "quirky contingencies" to be acceptable in an institution that is supposed to generate presumably impersonal constitutional law. Better leave such compromises to the legislative branch, according to Benjamin. Politicians, at least, are *supposed* to generate compromises that are reflective, if not duplicative, of public opinion. But which legislative branch and in which slice of policymaking time? In the end, after an impassioned defense of the ethics of reasonableness, respect, and integrity in political compromise by an expert on the subject, it is precisely the politics of the abortion debate that remains problematic. Who is the most likely to preside over political compromise on abortion—the philosophically inclined governor of New York, whom Benjamin cites repeatedly, or those less self-consciously subject to the unkind winds of pluralism, such as President Bush and the various candidates preparing for the November 1990 elections across the United States? Are these circumstances likely to couple splitting the difference with political expediency or sunder them further?

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Agitpop: Political Culture and Communication Theory. By Arthur Asa Berger. New Brunswick: Transaction, 1990. 216p. $24.95.

It's a daunting task to review a book by an author who, à la Philip Roth, has preempted the reviewer with his own autocritique, in which he dismisses all obvious criticisms. However, there is one central aspect of this discursive, entertaining introduction to the study of popular culture that does demand more consideration than the author gives it: his total reliance on a four-fold typology of political cultures developed by Mary Douglas and refined by Aaron Wildavsky, whose presence here is ubiquitous.

The Douglas-Wildavsky typology is based on the premise that (as Wildavsky puts it) in all complex societies answers must be given to two questions: Who am I? and How should I behave? Answers to the latter question may be strongly or weakly binding. Strongly binding group identities conjoined with pluralistic answers to the questions of identity are characterized as *hierarchical and collectivist*; conjoined with prescriptive answers to that question they are *egalitarian*. Weakly binding group identities conjoined with nonprescriptive answers to the question of identity produce *fatalism;* conjoined with more prescriptive answers to that question they produce a culture of *competitive individualism*. According to Wildavsky, as interpreted by Berger, these are the basic varieties of political culture. So entranced is Berger with this typology that he plays what he calls the Aaron Wildavsky Game with his students, who try to fit artifacts of popular culture under the various rubrics. The major part of *Agitpop* is an Aaron Wildavsky Game played with Berger's readers, who are invited to see professional football and the Iran-Contra Hearings as examples of hierarchical political cultures in action; the human potential movement and a music video, *Sledgehammer*, as individualistic; the television show *Max Headroom* and Jewish humor as egalitarian; and professional wrestling and the Arnold Schwarzenegger film, *The Terminator*, as fatalistic.

Individual readers will have their own reactions to these chapters. To my mind, only the last section, on fatalastic political cultures, justifies the Wildavsky Game. The other analyses mix psychoanalytic and sociological insights to produce quite obvious results that when translated into Wildavsky's schema, sometimes seem awry. Thus, the interrogation of Oliver North is presented as a conflict between hierarchically oriented congressmen and a competitive individualist—which, as a description of a career Marine colonel whose favorite word is *orders* and who expounds a strikingly authoritarian version of the political system, seems quite questionable.

What, in any event, is the point of the Wildavsky Game? In an exceptionally lucid and happily eclectic section, "Communications Theory," Berger gives a clear answer. (This section stands by itself and could usefully be assigned to undergraduates for its accessible descriptions of psychoanalytic, semiotic, Marxist, and other versions of cultural communications analysis). "Cultural theory," he writes, "is not deterministic at all; it posits the possibility of movement and assumes a thinking 'actor' who assesses payoffs and is generally capable of moving from one political culture to another (except for the fatalists, that is)" (p. 141). Throughout, it seems that the implicit alternative to the Wildavsky Game is some kind of alleged Marx (or Althusser?) Game, in which a deterministic version of cultural critique posits most social actors as victims, fixed into place by ideological manipulation like specimens at the end of a pin. Any such sociology of knowledge is certainly problematic in the extreme—but perhaps not more so than what sometimes seems the unalloyed voluntarism of Douglas-Wildavsky-Berger. If there is in any sense a dominant culture that monopolizes the space in which public communication takes place and is resistible only with great effort, we will never discover it by limiting analysis to the categories of the Wildavsky Game. In fact, to play the Marx Game for a moment, the Wildavsky Game, with its multiple political (sub)cultures and freely shifting commitments looks remarkably like the ideology of pluralism in a new guise: all's well in the social order, no cultural domination here, just us rationally choosing citizens. It is interesting, too, that in this methodological section hierarchical collectivists suddenly become "hierarchical elitists"; and Berger momentarily falls into a language of political abuse reminiscent of Douglas and Wildavsky's *Risk and Culture* (1982): "Many affluent hierarchical elitists . . . are statists who believe in the use of governmental institutions to solve social and political problems" (p. 169). If that kind of game is the alternative to neo-Marxian versions of ideology critique, the latter look much more attractive.

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Book Reviews: Political Theory

The Federalist: Design for a Constitutional Republic. By George W. Carey. Urbana: University of Illinois Press, 1989. 181p. $22.95.

Americans' interest in *The Federalist Papers* has always been marked by a concern for the character of the American polity. Today the issue of the republic's character is at the center of a great controversy raging among scholars of the American founding; and at or near the center of this controversy rests a neatly bound edition of *The Federalist.*

Carey's book represents an attempt to confront this question on Publius' own terms. He treats Publius as one who speaks in a single voice and vehemently criticizes interpretations that overlay Publius' text with latter-day populism, progressivism, or positivism. In so doing, Carey both illuminates and avoids some of the most glaring deficiencies of twentieth-century scholarship on *The Federalist.* His analysis would be sharper and more complete, however, if the more recent scholarship done on *The Federalist* (by David F. Epstein, Thomas L. Pangle, William B. Allen, and Charles R. Kesler for example) were given more serious consideration.

The main focus of Carey's theoretical analysis is what he calls the filter problem, or how republican government can refine and enlarge the public views so that justice and the common good will prevail. The emphasis is appropriately placed, for this problem and Publius' solution to it are at the heart of his teaching. Carey understands Publius' republican scheme to depend on the extension of the territory, which is necessary to achieve his two-part solution: (1) the elevation of fit characters to office and (2) the multiplicity and diversity of interests in society. The second part of this solution tends to deter the formation of decisive popular majorities, that is, majorities that continually coalesce in support of, or in opposition to, clearly defined issues. The system of government Publius envisioned would instead be founded on permissive majorities, which grant to the representatives a wide latitude in both selecting and resolving public issues. In view of this, Carey argues that they key to the filter problem is the U.S. Congress. Because the representatives are so distanced from their constituents and also because of the existence of an impartial, mediating group in Congress, the national legislature would not be likely to succumb to factious impulses.

Despite these disinterested congressmen, auxiliary precautions are necessary to control the government. Separation of powers, checks and balances, and bicameralism are intended by Publius to prevent the branches of government (especially the legislature) from overstepping their constitutional limits and thereby placing themselves in a position to tyrannize over the people. However, Carey asserts that it must be understood that Publius did not intend these auxiliary precautions "to apply against factious majorities, but rather against the rulers" (p. 69).

Carey's treatment of *The Federalist* is not a rehash of old interpretations. Unlike most prior work, his analysis demonstrates that Publius' political scheme provides a deterrent to direct popular will *and* a constructive role for public leaders in attaining republican ends. The difficulty with Carey's interpretation, however, is that in order to make his case for permissive majorities and decisive legislative control, he must underemphasize Publius' own words concerning who actually rules or prevails in the regime. He pays little attention to Publius' view that the creation of a governmental will independent of the will of the majority is unrepublican. Carey does clearly recognize *The Federalist*'s teaching that "the deliberate sense of the community is to prevail" but nonetheless treats Publius' community as if it would very seldom possess a will or a sense of its own at all.

This leads the author to a partial and misleading view of separation of powers. Regardless of the numerous warnings in *The Federalist* that the legislative branch is closest to the people and that the House of Representatives, particularly, may well speak "the known and determined sense of a majority of the people" (*Federalist,* no. 58), Carey nevertheless insists that the auxiliary mechanisms discussed in *Federalist,* number 51) are not intended to protect against majority faction working its wiles through the legislature. While it is true that in *Federalist* number 51 Publius does not go beyond a discussion of how these mechanisms are to control the government, it is also true that in this same essay Publius clearly announces that he will not "undertake a full development of this important idea." At the least, the author might have been more cautious about drawing emphatic conclusions from this incomplete exposition in *The Federalist.* Moreover, on this occasion it may have been helpful to consider a later statement of the author of *Federalist* number 51, James Madison, written only a few months following the completion of *The Federalist.* In a letter to Thomas Jefferson of 17 October 1788, Madison stated that "the invasion of private rights is *chiefly* to be apprehended, not from acts of Government contrary to the sense of its constituents, but from acts in which the Government is the mere instrument of the major number of the constituents. This is a truth of great importance, but not yet sufficiently attended to" (Robert A. Rutland et al., eds. *The Papers of James Madison* [Charlottesville: University of Chicago Press, 1977], 11: 298.)

The distance Carey places between the people and their representatives is simply too great, and the distinction drawn between decisive and permissive majorities much too severe for the text to bear. The overemphasis on Congress, the deemphasis on the will of the national majority, the almost total neglect of the executive, and the treatment of Publius' federal principle as gratuitous leave unsatisfied the author's goal to understand and present Publius on his own terms.

In examining the judiciary, the author's spirited critique of the contemporary Supreme Court is unambiguous. Here Carey is at his best. He demonstrates clearly the disjunction between Publius' intention and current practice. Whether one agrees or disagrees with the willful actions of some contemporary justices, Carey is persuasive on this point: these actions represent an abandonment of *Federalist* constitutional doctrine.

This work culminates with the problematic question of the place of virtue in Publius' conceptual framework. Carey's exhortation to remember the task of Publius sobers the reader. After all, he argues, *The Federalist* was not meant to be a complete and systematic political philosophic treatise but rather a political tract designed to secure ratification of the proposed Constitution. Thus, we should not be surprised that Publius does not raise the "complex and potentially contentious" issue of the fostering of virtue in the polity. It is to Carey's credit that he is able to drive this point home and still retain a proper regard for Publius' brilliant contribution to the theory of republicanism. Unfortunately, however, he ultimately leaves the reader unclear about whether the discussion of the cultivation of virtue was simply outside the purview of *The Federalist* or was instead a matter with which Publius was unconcerned. The treatment given to Publius' proper place in the dispute between the ancient and modern political philosophers, although brief, would better await a fuller discussion of this important question. Not only the question of means but also the question of the ends of the new republic must be examined before conclusions regarding the classical or modern character of Publius' conceptual framework can be rightfully drawn.

Because Carey does not substantively address the ends Publius envisions—and because he claims that "there are no appeals to abstract rights" in *The Federalist,* which is simply not true (see *Federalist,* nos. 40 and 43)—he unwittingly makes Publius into something of a positivist. The careful reader will ask whether this political order, informed by the will of the legislature, which is in turn informed only by the morality of convention, possesses any rational defense against rule by faction. We remember that Publius teaches that a faction is a group identified not only by its actuating cause but also by the fact that it *ends are unjust.* Publius' discussion makes absolutely clear that republican ends are *not* derived from positive law or convention. Surely, they are sown in "the transcendent law of nature and of nature's God," which, we are told, informs us of the purpose of government.

COLLEEN A. SHEEHAN

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The Oppositional Imagination: Feminism, Critique, and Political Theory. By Joan Cocks. New York: Routledge, 1989. 245p. $45.00 cloth, $13.95 paper.

In *The Oppositional Imagination* Joan Cocks attempts to contribute to what has become, in recent years, a well-established and ongoing conversation between feminism and more mainstream political theory. Her topic is a vital one: power. But the kind of power with which she is concerned is not that of the state over its subjects. Rather, she is concerned with the power of what she calls "cultural-political regimes." Such regimes do not operate "through the imposition of legal right and the monopolization of the means of physical violence" (p. 39) but through the operation of language, symbols, and other cultural artifacts.

Cocks sets herself a double task. In part 1, "On Theory," she attempts to develop an account of the workings of such cultural power in general. To do this she draws on some strands of critical theory—above all, the work of Gramsci and Foucault—to develop an account of power as multidimensional and multicentered; as productive and positive (as well as oppressive); as hegemonic (yet capable of change through multiple, small "molecular" transformations). In part 2, she attempts to apply this general account to the specific domain of gender relations, which she calls the "regime of Masculine/feminine." By doing this she hopes to develop an engagement between critical theory (as she construes it) and feminism. Her project is, she says, to write "from two different intellectual positions" and "for two different audiences" (p. 21), with the goal of thereby enriching the theoretical discourse and contribution to political practice of each.

Cocks' project is an ambitious one; but ultimately it does not add a great deal to our understanding of power, either in general or between the sexes. Part 1 offers an elegant weaving together of ideas from Gramsci, Foucault, and others; but in part 2 one expects Cocks to break new ground. However, much of part 2 is taken up with a critique of radical feminism that, while convincingly argued, adds little to a host of other treatments available. Similarly, Cocks' alternative to radical feminism—an analysis insisting that there is a multiplicity of divergent scenarios for gender relations and resistances—is valid but hardly novel.

Cocks argues that while radical feminism's "exposure of the intersection of politics, culture, and sexuality" (p. 11) is a stroke of genius, it lacks an adequate theory of power. Radical feminism has merely produced "the reverse conceit of its enemy" (p. 136). It has uncritically accepted the dominant cultural portrayal of sexual relations as an accurate account of the reality. As a result, it has fallen into "theoretical manichaeism" (p. 123), dividing the world into men and women seen as inherently powerful and powerless, active and passive, evil and virtuous, respectively. It thus misses the complexity and multidimensionality of gender and gendered power relations. Cocks's argument is cogently made and convincing, yet it adds little to numerous existing critiques of radical feminism. Similarly, her insistence, against radical feminism, on "the deep meaninglessness of the body" (p. 148) is an important point but one that numerous other antiessentialist writers, particularly those of a poststructuralist turn, have already made.

It is puzzling that Cocks chose to try to articulate *radical* feminism with her general account of power. For radical feminism's theory of power is, as she shows, so simplistic that to bring her elaborate critical apparatus to bear on it is rather like cracking a peanut with a sledgehammer. Moreover, the bulk of the radical feminist writings on which Cocks draws (such as those of Mary Daly, Adrienne Rich, Andrea Dworkin) were published in the late 1970s. Thus, Cocks's lack of discussion of more recent feminist theory is also puzzling. Particularly striking is her virtual silence about poststructuralist feminism, for her own work has clear affinities with some of it. Authors such as Nancy Fraser, Judith Butler, and Iris Young (to name but a few) have already incorporated into feminism the antiessentialism and the Foucauldean notions of power and sexuality on which Cocks draws. Their work has *already* constructed bridges between the "two different intellectual traditions" and "two different audiences," which Cocks belatedly labors to connect.

SONIA KRUKS

Oberlin College

Montesquieu's Comparative Politics and the Spirit of American Constitutionalism. By Anne M. Cohler. Lawrence: University Press of Kansas, 1988. 210p. $22.50.

In the midst of the growing literature that articulates the principles on which the U.S. republic was founded, Cohler presents a careful view of Montesquieu's treatment of spirit and comprehension of the complexities of politics in context. Instead of trying to capture the original intent of the founders in a net of particular principles, the analysis provides a corrective to forced readings of Montesquieu and to excessive efforts to transform the deliberations into theoretical forays. Cohler emphasizes Montesquieu's effort to establish a political life between the limits of universal nature and passions and "the universality of God, or the best." The goal of the founders was a free government that avoided the despotic, and they received guidance from Montesquieu's *Spirit of the Laws*; that is, "Montesquieu has devised a context for a modern prudence" (p. 191).

Cohler sees her own work as *additive* to other current theoretical explications of the forces affecting the founders, rather than as an attempt to displace them: "This is not to say that this is the whole account of the Founding; rather it casts another light upon the thought behind the shape of the Constitution. Political action never has quite the consistency of political theory" (p. 10). Her analysis tempers the debates over original intent that now often accompany the expansion of the current literature.

Cohler commences her assessment of Montesquieu carefully and sensitively, drawing much from her work translating *The Spirit of the Laws* with Basi C. Miller. She draws attention to an obvious point often missed in studies focusing on modes of single principled theories: the obligation to study the path that Montesquieu himself laid out (pp. 5-6). Cohler asserts that this is how the *Federalist* and Tocqueville gained from the book (p. 8). She avoids philosophical reconstructions in favor of following Montesquieu's own structure of presentation.

The importance of *spirit* is developed well throughout the book, especially in chapter 3. Cohler explores Montesquieu's contrast between the natural order of laws and the order of the relations of things (p. 34). She builds on his division of human inclinations into "active and passive elements—the spirit and the heart. Spirit has to do with action from within a person toward something. The passions happen to us" (p. 49). Thus, she points out that in Montesquieu's view, we respond to the necessities of passions but can choose directions through the activity of spirit (p. 50). Passions (necessities) are individual, but spirit provides something that can be shared with others (p. 55). Changes in spirit are revolutionary, in Cohler's view, because they "indicate a fundamental change in spirit, in the shape of the spirit" (p. 56). This explication, of course, reminds one of Aristotle's notion of revolutions as changes in the regime's constitution. She carefully explicates the first portion of the text to show that the criteria for "political" understanding and assessment should be "spirit and moderation, rather than law and right" (p. 64).

The book's most interesting chapter deals with moderate and free government, using the Constitution as the exemplar (chap. 7). Cohler argues that the proposers of the Constitution saw the problem of legislation through Montesquieu's terms: (1) the government they sought was free because it was moderate and under law but pursuant of its ends, not limited and guaranteeing only natural rights; (2) the urge to moderate government must be resolved with republican representation that ensures deliberative rather than partisan rule; and (3) the structure of the institutions must be the source for achieving these purposes (p. 149).

If there are quibbles, they do not detract from the book's importance. First, the chapter dealing with Tocqueville's democracy (chap. 8) does not seem directly related to the overall purpose of the book. Though interesting, the space could have been used to link the important major theme to U.S. constitutionalism more comprehensively. Second, the importance of the arguments concerning the "in-between" could be further amplified by tracing more of the classical origins. One might be drawn not only to Eric Voegelin but, from within Cohler's text, more directly and importantly to Aristotle and his congruence with the founding. Cohler's apt summary phrase, "the best possible in their circumstances" (p. 190), clearly establishes a direct parallel with the knowledge required of Aristotle's statesman and focuses our attention, in considering the founders, on the constitutional form of rule rather than on democracy, especially in light of Montesquieu's remark that the ancients called balanced governments *"police"*, 'polities' " (p. 18).

Quibbles aside, the book is a solid contribution to our understanding of both Montesquieu and the U.S. founding.

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Book Reviews: Political Theory

For Anarchism: History, Theory, and Practice. Edited by David Goodway. London: Routledge, 1989. 278p. $17.95.

Kropotkin and the Rise of Revolutionary Anarchism 1872–1886. By Caroline Cahm. Cambridge: Cambridge University Press, 1989. 372p. $49.50 cloth.

Anarchists have typically been seen as either naive idealists or mindless bombthrowers. In truth, most have fallen somewhere between these two extremes as they dealt with the theoretical demands of anarchist ideals and the practical necessities of anarchist activism. In different ways, the anthology *For Anarchism* and Cahm's monograph on Peter Kropotkin address this dilemma of anarchist praxis.

Goodway's anthology attempts to break the dilemma into its constituent parts, theory on the one side and history and practice on the other. Luckily, there is considerable overlap. For political theorists, the four articles in the theory section are likely to be of most interest. Daniel Guerin offers a brief summary of the bad blood between Marxism and anarchism, unfortunately without saying much about their theoretical differences. Alan Carter's piece is more satisfactory, for he develops an "anarchist theory of history" that is more parsimonious, and explains more, than the Marxist model. Turning Marx on his head, Carter argues that the state shapes the relations of production in order to guarantee a surplus that it can extract to build up the "forces of coercion." In short, Carter considers the institutional needs of the state, not its "relative autonomy," a perspective that offers a simpler explanation of why the Bolshevik state did not "wither away" than do the many volumes of Marxist apologetics. Carter's voluntaristic antithesis to Marxist determinism finds an echo in Peter Marshall's survey of anarchist theories of human nature. Marshall eventually challenges the concept of a fixed "human nature" altogether, suggesting that humans are "both the products and agents of history" (p. 141). Robert Graham takes this a step further in questioning how and why people would cooperate in the absence of state coercion. Focusing on "self-assumed obligations," he draws connections between anarchism and laissez-faire liberalism, particularly the potential role of contract in an anarchist society.

Although the theory articles are interesting, it is the remaining articles in *For Anarchism* and the book on Kropotkin that really tackle the question of praxis. Nick Rider's article on the 1931 rent strike in Barcelona paints a picture of anarchists there as practical strategists responding to the problems of the urban working class in a rapidly growing city. Carl Levy's survey of Italian anarchism from the 1860s to the 1920s suggests that anarchists, though a minority within the Socialist movement, had a disproportionate influence, particularly on syndicalism and council communism. In the present day, anarchism as a movement and even as an explicit ideology is minuscule, although its influence is widely felt in some circles. Tom Cahill suggests that "contemporary co-ops . . . are subtly imbued with the anarchist spirit" (p. 251) even though he attributes the dramatic growth of co-ops in Britain in the late 1970s and early 1980s to the legalization of common ownership and the provision of advice and financial support from national and local governments. Murray Bookchin, the preeminent anarchist theorist of the last twenty years, offers a much more strident and convincing thesis connecting anarchism to new social movements such as environmentalism, feminism, antimilitarism, and community control groups. Decrying the drift of the New Left to conventional politics and the irrelevancy of a Marxism confined to the "professoriat," Bookchin sounds the recurring themes of this volume: "If there is to be *any* Left today or in the future, it will have to come from various forms of eco-anarchsim in conjunction with the new social movements on both continents" (p. 274).

Cahm's book on Kropotkin deals in the greatest detail with the dilemma of praxis, focusing on one of anarchism's greatest theorists and revolutionaries. Cahm's intentions are to rescue Kropotkin from undue identification with either "propaganda by deed" or bookish theory. Drawing upon the proceedings of various anarchist conventions, Kropotkin's articles in *Le Revolte*, and the writings and activities of anarchists in most of the countries of Western Europe, Cahm makes a sophisticated case for rooting Kropotkin's theories and strategies firmly in the experiences of continental anarchism. Unfortunately, the result of such conscientious research is a book that relies on long quotations and overlapping narratives and presumes considerable knowledge of late-nineteenth-century socialism and anarchism—or at least considerable willingness to flip back to the endnotes. Nevertheless, Cahm makes some audacious, if muted, claims. For example, she says that anarchist communism (usually associated with Kropotkin, its most tireless advocate) actually predated his writings and that he turned to anarchist communism for strategic reasons, namely, to inform and enhance the propaganda effect of anarchists' "acts of revolt." Eventually, Kropotkin argued that if anarchists were going to engage in terrorism, it ought to be economic terrorism, pointing the way to the eventual revolutionary act of expropriation. The common thread underlying both books is revealed clearly in Cahm's analysis of Kropotkin's 1880 series of articles, "The Spirit of Revolt," whose principal theme was the "vital importance of a strong and close relationship between theory and action" (p. 166).

In short, anarchists both in the nineteenth century and now have tried to find strategies consistent with their ideals, a struggle that at its best encourages creativity and flexibility. As the crisis of both parliamentary and revolutionary socialism continues, it is thus useful to reexamine—or rather (in keeping with the spirit of these books (reapply and revise—the historic third strand of socialism, anarchism.

FRANK H. BROOKS

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Pulpits, Politics, and Public Order in England, 1760–1832. By Robert Hole. Cambridge: Cambridge University Press, 1989. 326p. $54.50 cloth.

Robert Hole sheds new light on an important subject: the secularization of English political thought. He shows how the "theoretical justification" for political action—on both sides of the major debates in English politics—"underwent a very radical change" from 1760 to 1832 (p. 256). This development involved "crucial changes in the way the English related their religious beliefs to their views of politics and society" (pp. 1–2). The French Revolution played a decisive role in the process by dramatically intensifying and accelerating "trends which had been prefigured in the 1780s" (pp. 248–49).

Hole defines the categories of analysis in a methodical way. First, he divides political thought into political theory and social theory. Political theory addressed the traditional, "speculative" concerns of political philosophy: the origins of government, the sources of authority and political obligation, the right of rebellion—in short, the "philosophical themes established during the Revolution Settlement" of 1689 (p. 59). Social theory, on the other hand, was concerned with the nature of society; it stressed the role of religion in restraining human behavior, the importance of a divinely sanctioned social hierarchy, and "the need for the poor to be content with their lot" (p. 84)—the agenda established in reaction to the French Revolution. Hole then distinguishes between religious argument and secular argument in political thought. Religious argument "consciously depended for its effectiveness" on belief in God and acceptance of the authority of scripture; without God, the entire theoretical construction would collapse (p. 7). In contrast, secular argument mentioned God, if at all, "only in a cosmetic way"; "the structure of the argument remained intact whether or not one believed in His existence" (p. 7).

Hole develops his thesis within this methodological framework. Political theory in 1760, whether divine right or contractarian, consisted primarily of religious argument; it revolved around the notion of God-given rights: the rights of the monarch or the rights of the people (p. 249). By 1832, the concept of God had been transferred from the center to the periphery of political theory (and in a few cases expelled altogether); "humanly perceived utility" had replaced "divinely imposed duty" and revealed truth as the foundation for political prescription (pp. 249–50). Hole's study of the printed sermons and speeches of clergy from various denominations and on both sides of the political debates shows that this change occurred "within the churches" themselves (p. 7). It was, in fact, William Paley, the influential Anglican Archdeacon of Carlisle, who, writing in the 1780s, contributed more than anyone else to the process. Paley's utilitarianism "lay at the heart of the secularization of English Christian political theory"; his "utilitarian theory of political obligation . . . denied God a structural role and left him only a cosmetic one" (pp. 74–75, 82).

Hole cites as both an important development accompanying the secularization of political theory (p. 7) and an effect of the secularizing process (p. 84) a shift in clerical attention from political theory to social theory and a corresponding movement of the religious argument from the former to the latter. Christian writers had always been concerned with social theory; the role of religion in maintaining social order was an age-old theme. However, with the advent of the French Revolution and the perceived need to defend "not just the established political and social order, but also an intellectual and conceptual universe" against the atheistic doctrines of the Enlightenment (p. 144), social theory became the "central arena" of debate as well as the repository of religious argument (p. 98). This complex process became irreversible by 1793 and was completed by 1832 (pp. 102, 256). By then, "the church had largely abandoned the exposition of a theologically based political philosophy and was devoting itself to social theory," where "the supporters of revealed religion and a theocentric society" now concentrated their efforts (pp. 246, 256).

There is much to admire in Hole's book. The scholarship is careful and thorough, the exposition clear and well organized, the argument balanced and persuasive. A bibliographic appendix conveniently places the book in the context of related scholarly productions (most notably, the works of J. C. D. Clarke). Problems, on the other hand, seem marginal. For example, Hole does not fully substantiate the allegation that Jeremy Bentham's philosophy has "done more than anything else to undermine the centrality of religion and to destroy it as a major intellectual force in modern society" (p. 203); the critique of Bentham, however, is not central to Hole's thesis. Moreover, the text does make substantive demands on the reader. It presumes familiarity with English history (e.g., the Wilkes Affair), the vocabulary of theological disputation (Socinianism, Arminianism, Erastianism, etc.), and theoretical distinctions rarely encountered beyond graduate school examinations (e.g., rule utilitarianism versus act utilitarianism). A few explanatory notes would help, but their absence is a minor inconvenience that should not deter scholars from engaging this rewarding exercise in the history of political thought.

STEVEN M. DWORETZ

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Liberalism, Community, and Culture. By Will Kymlicka. New York: Oxford University Press, 1989. 280p. $38.00.

Will Kymlicka provides an important defense of contemporary liberal theory from communitarian attacks and develops the implications of liberalism for the issue of collective rights. He responds to the charge that liberalism is excessively atomistic and individualistic—that it misunderstands our social being and forgets that community is necessary to freedom. Kymlicka shows that although liberal commitments are compatible with the recognition that people develop through social interaction, liberalism must reject the notion that particular ends are constitutive of the self. Our interest in leading a good life can lead us to question what we currently believe to be good, and the good life must be lived according to our own beliefs and values. Since we can always imagine ourselves with different ends, our self-understanding confirms what a full conception of freedom presumes, namely, that cultural practices can always be examined and rejected. However, Kymlicka does not take seriously enough the possibility that individuals may sometimes need to transcend their own society to reexamine their projects.

Defending the idea of individual rights against the Marxist critique, Kymlicka insists that such rights are compatible with affective ties and that the attribution of rights is a way of expressing respect for others and for oneself. At the same time, he argues that individual rights must sometimes be limited in order to protect *collective* rights. Rawls and Dworkin assume that rights belong only to individuals because, according to Kymlicka, they orient their theories toward political communities that are coterminous with one cultural community. Developing the implications of liberalism for multicultural communities, Kymlicka discusses the devastating implications on minority *communities* of rights that liberals generally attribute to *individuals* (such as the rights to personal property, mobility, and political participation). His main example is the plight of aboriginal peoples in Canada. He argues that since the range of options about possible ways of life is structured by one's cultural heritage through linguistic and historical processes, a rich cultural structure is necessary for meaningful decisions about the good. The cultural structure is therefore a primary good, though Rawls did not identify it as such; and to protect minority cultures it may be necessary to limit the rights of members of majority cultures to live, to own property, and to vote in minority communities.

Kymlicka realizes that his argument can be abused by those who want to protect not only the cultural structure of a minority community but also their own particular views of what that structure should be. He therefore insists that the given character of cultural communities must be permitted to change through the choices of individuals. Of course, an important question—which Kymlicka will perhaps address in future work—concerns the proper way to distinguish the fundamental cultural structure of a community from individual preferences about that structure. Nonetheless, *Liberalism, Community, and Culture* provides a provocative analysis of the place of cultural membership in liberal theory, as well as an excellent elaboration and defense of liberal principles.

ROBERT B. THIGPEN

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Social Justice Reconsidered: The Problem of Appropriate Precision in a Theory of Justice. By David Mapel. Champaign, IL: University of Illinois Press, 1989. x + 169p. $24.95 cloth.

The chief aim of Mapel's *Social Justice Reconsidered* is to redirect the debate about theories of justice activated twenty years ago by the publication of Rawls's book. As Mapel sees it, the core of this debate, which pits Rawls and his "imitators" against critics such as Sandel and Walzer, is the question of how much precision we should expect in theorizing about justice. The main body of this book is a set of critiques of various views of justice, each of which, Mapel says, demands more precision than can be achieved, requiring measurements and comparisons that are either literally impossible or too difficult to be practical. Mapel considers in turn theories of desert and the accounts of justice provided by Dworkin (especially in "What Is Equality?"), Ackerman (*Social Justice in the Liberal State* [1980]), Galston (mainly *Justice and the Human Good)* and the later Rawls.

Mapel claims that all of them demand too much precision; but he does take Rawls's views as a step in the right direction, toward a kind of proceduralism. If we cannot evaluate allocations directly with any precision, we might still at least evaluate the procedures that bring them about, such as the basic institutions of market and democracy. This line of thought is basic both to contemporary revivals of classical liberalism (e.g., Hayek) and to contemporary participatory democrats (e.g., Barber), which urges us to replace philosophy with a purified politics. Without too much argument at this stage, Mapel sides with the participatory democrats and ends with some kind words for the ideal of complex equality as enunciated by Walzer.

This is a short book, written in sharp and readable prose, containing novel and interesting interpretations of the various theories considered. But these are incidental to the main argument: that the level of precision possible in theories of justice is *the* central issue, that this issue can be resolved, and that therefore (1) social justice is a vague idea, and (2) we should reject current fashions for "preference functions" and "hypothetical contracts" in favor of something like an ideal of complex equality à la Walzer, rooted in the historically given cultural heterogeneity in which we live. Unfortunately, the longish discussions of Dworkin, Ackerman, Galston, Rawls, and company leave Mapel with little space actually to establish these claims.

It is by no means clear that the level of precision is such a crucial issue. For some, the chief problem with Rawls (and his "imitators") is their search (largely abandoned by now, in any case) for some kind of "Archimedean standpoint." More plausibly, the trouble lies with any attempt to construct an all-encompassing theory of justice. Neither of these has much to do with precision. In any case the *real* avant-garde of precision in political theory (Harsanyi, Gauthier, Sen) is completely absent from the present book. But it is also possible that I have not fully understood what Mapel means by *precision.* He complains about a *style* of theorizing (with hypothetical contracts, preference functions, and so on). This style is better represented by Harsanyi or Sen than by Dworkin. But Mapel directs his arguments against a very special case of precision: "precise judgments about what is due each member of society" (p. 2). But the people with preference functions (and other kinds of mathematical horrors) are not often guilty of precision on *that* front. Sen's work, for example, develops a kind of pluralism of principles (liberal rights versus welfare) that should be appealing to a Walzerian.

In any case, however, it is unlikely that there is *one* level of precision appropriate to the subject of social justice. Some aspects of the subject can surely be made very precise indeed (resolved by mathematical proof, say, as in Sen's "impossibility of a Paretian liberal"); others are in fact inherently vague. If this is true, Mapel's strategy in the present book is not likely to be fruitful, for the simple reason that it asks the wrong question.

KAROL SOLTAN

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Market, State, and Community: Theoretical Foundations of Market Socialism. By David Miller. Oxford: Clarendon, 1989. 359p. $59.00.

Ideas ebb and flow to a different rhythm from events. In the midst of a transition from socialism to capitalism at least as dramatic and apparently conclusive as anything Marx ever had in mind for the reverse, it might be thought that socialist ideology would by now be dead and buried. In fact, the pro*capitalist* case of the Hayeks and the Nozieks has been chipped away at to such good effect by radical critics over the last 15 years that it now lies in ruins. David Miller's book is both a distinguished contribution to this task of demolition and a persuasive work of socialist reconstruction built upon the remains of the libertarian argument.

Miller begins with a useful, if standard, destruction of the libertarian's freedom claims: the libertarian can argue that capitalism maximizes freedom only by adopting bizarre definitions of freedom that overlook all the ways that capitalism infringes freedom while including all the ways that state socialism infringes freedom. What Miller does broadly accept in the libertarian case is the promarket argument from efficiency and consumer sovereignty, which he shows in chapter 5 to be robust against the criticism of the leftist critics who dwell on the undoubted tendency of capitalism to create inauthentic appetites and needs. This leads Miller to distinguish rather sharply between systems of commodity exchange and systems of private party ownership, from which it follows that arguments in favor of the former are not necessarily arguments in favor of the latter. A viable market socialism would then preserve the advantages of markets while overcoming the drawbacks of unequal private property regimes in respect of welfare and justice—on which topic Miller gives an incisive exposition of the Analytical Marxist view that "the worker's needs and the employer's command of resources together create the power relationships that culminate in exploitation" (p. 195).

One of the principal duties of the state under market socialism would be to preempt the exploitation that might always reassert itself among producer's cooperatives linked by market relations; but the content of socialist politics (and thus the role of the socialist state) is not exhausted by the administration of distributive justice. Interests must be formulated in a process of dialogue as well as reconciled in a process of negotiation; and Miller is commendably reluctant to reduce a theory of democracy to either its procedural dimension (Arendt's mistake) or its interest aggregation dimension (the liberal mistake). The third element of the institutional triad—community—now enters the picture not just in recognition of a human need to identify with whatever social order prevails but as a condition for the ongoing dialogue of the citizens of the democratic socialist republic.

Miller is at his very best (which is very good indeed) when relating abstract principles and formal models to the practical requirements of real-life institutions, at which Analytical Marxists and others have fallen short. There is much to learn in his discussion of the role of the socialist states as an arms-length investment banker for the workers' cooperatives and the tendency of cooperatives to suffer technologically in competition with capitalist firms, which are able to avoid paying the intramarginal product to their employees (chap. 3), as also in his views about critical nationalism (chap. 9) and the need to set the practice of multicultural education in the context of common citizenship (chap. 11) or his discussion of the content of a socialist Bill of Rights (chap. 12).

I have one minor and two major reservations to Miller's achievement. The chapter 4 discussion of altruism seems to fall between two stools: it is sufficiently technical to turn off the non-technically minded while being insufficiently technical to turn on the technically minded. It could have been omitted without much loss to the general line of argument. A more significant reservation in his consistent use of sexist forms of language. This is just not good enough in an egalitarian text of the 1990s. A final reservation is theoretical. The three institutions of market, state, and community still stand to each other somewhat in the relationship of a functionalist sociology: a suitably reformed market is the economic power house, whose energy must be directed by a suitably democratized state machine in pursuit of the social values derived from a suitably reconstructed community of participation—all working together in a well-balanced market socialist whole. Less prominent is the perception announced in a striking, yet unfulfilled, paragraph stating that market, state, and community are alternative modes of social exchange, between which choices lie for the delivery of various goods and services (p. 18). Miller thus neglects either to consider in any detail decentralized (but nonmarket) forms of state provision that honor consumer sovereignty by administrative means or to explore the scope and limits of the systems of reciprocal aid that constitute the real political economy of communal organization. Miller is surely right to be impressed by the market; but in these respects he may be overimpressed—state and/or community might be able to do more of the work Miller reserves for the market.

Yet the book overall offers a comprehensive and lucid statement of the case for market socialism in the light of all the best academic discussion of the last decade. Miller's statement is important beyond the academy, as the world gropes towards social systems that would unite the dynamism and flexibility of capitalism with the standards of social justice and personal satisfaction envisaged in the socialist ideal, however often these standards have been flouted in socialist practice.

ALAN CARLING

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Justice, Gender, and the Family. By Susan Moller Okin. New York: Basic Books, 1989. 216p. $19.95.

In her second book, Susan Okin continues and extends her critique of how political theorists have thought about gender that she began in her first book, *Women in Western Political Thought* (1979). She continues the critique by a careful and thoughtful examination of recent writings about justice by such political theorists as John Rawls, Michael Walzer, Robert Nozick, Alasdair MacIntyre, Michael Sandel, Allan Bloom, Bruce Ackerman, and Roberto Unger. Okin defines *gender* as "the deeply entrenched institutionalization of sexual difference" (p. 6). She uses her liberal feminist sensibilities to make a positive theoretical argument that the desirable end of a genderless society can be best achieved by applying standards of justice to the family. The result is an original and provocative book of great importance.

Okin, like all good theorists, invites her audience to see the world differently. Citing issues of "sex discrimination, sexual harassment, abortion, pregnancy in the workplace, parental leave, child care, and surrogate mothering," Okin claims that "there is clearly a major 'justice crisis' in contemporary society arising from issues of gender" (p. 7).

Okin's argument proceeds on several levels. At the most general level, she argues that justice is a relevant standard to use when looking at what happens in families. She criticizes views of the family as naturally affectionate and beyond justice (Sandel) or naturally unequal (Bloom). She further argues that justice should be a standard applied to the family because without a consideration of the justice of the family, (1) gender inequality will persist, (2) equal opportunity for children of both sexes will be thwarted by the gender roles they continue to learn in the family, (3) the possibilities a more just society will diminish, since it is crucial that children learn about justice in the family.

At another level, Okin works to establish which standards of justice are appropriate to apply to the family. She considers and rejects traditional arguments about justice that grow out of shared or communal values (criticizing Walzer and MacIntyre) and libertarian ideals of justice (Nozick), as well as arguments by Unger and Ackerman. In the end, two sets of ideas are most appealing to Okin: Walzer's argument about separate spheres is useful for considering gender discrimination as it spills over from family to other spheres of life; but Okin ultimately champions a modified version of John Rawls's notion of justice as fairness. Okin offers a significant modification of the assumptions of the original position, but she then argues that Rawls is right to see the family as the institution within which we learn about justice, which should therefore be just.

Using a Rawlsian sense of justice as fairness, Okin then investigates the relationship of public and domestic life and how pervasive inequalities in work and socialization make women and children vulnerable in families. After these more empirical chapters, Okin concludes with policy recommendations to enhance levels of justice in contemporary families. Although Okin argues for the ideal of genderless families, she also claims that her proposals do and should serve to preserve traditional families in the present transitional state of diverse family life.

About half of this book is a dialogue with contemporary theorists of justice; the other half looks critically at the empirical condition of families in the United States today. Okin's work is careful and deserves serious attention. Some may not be convinced that by refuting Sandel and Bloom, Okin has demonstrated that justice should apply to the family. Conservatives may find the argument supporting a genderless society unconvincing. Some feminists are likely to continue to debate the question whether fixing justice in the family is the most certain path to a more just and genderless society as well as whether these goals are admirable.

Much as Okin's first book was an opening salvo in the debate about the treatment of women in the history of political thought, this second book makes an argument to establish the importance of gender for those who wish to think about justice and the importance of justice for those who wish to think about gender. For committed feminists looking for clear thinking about the family, for committed antifeminists looking to see counterarguments carefully developed, and for political scientists who wonder whether feminist scholarship is really worth reading, Okin's book will prove extremely valuable.

JOAN C. TRONTO

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Identity Politics: Lesbian Feminism and the Limits of Community. By Shane Phelan. Philadelphia: Temple University Press, 1989. 206p. $29.95.

Shane Phelan's *Identity Politics: Lesbian Feminism and the Limits of Community* explores the difficulty with which lesbian feminism meaningfully counters liberal ideology in the United States. Throughout the text, the author defines lesbian feminism not only as a sexual preference but as a political identity and practice intent on discrediting certain precepts of the prevailing liberal ethos. These precepts are liberalism's characterization of the abstracted, autonomous, self-seeking human subject understood primarily in terms of individual rights and privileges, a characterization that separates the individual from, rather than binding the individuals to, the community. In *Identity Politics,* Phelan explains how the alternative, less privatized ethos that many lesbian feminists uphold might undercut the heterosexist, classist, racist ideology that liberalism consequently produces. She also offers an insightful, honest analysis as to why the political praxis emanating from lesbian feminism has failed to challenge the reigning liberal paradigm effectively.

To lesbians, identity politics and its promise of shared community has been oversold: this constitutes Phelan's central argument. Citing Foucault's insistence on identity as discursive by-product rather than ontological category, the author regrets lesbian feminism's dangerous propensity to disregard this concept and to revert to the romantic notion of an ontological identity. The unfortunate outcome of this reversion is a tendency to produce totalizing theories (of the human subject, of sexuality, of a collectivist politics) that view all political issues in terms of the dynamic between sexually oppressive and sexually oppressed. The author, of course, applauds the self-assertion of marginalized groups such as lesbians, whose voices she does not wish to silence and whose identities she takes seriously; still, she recognizes how frequently identity theory reverts to mechanistic, totalizing dogma. Moreover, this specific reversion to ontological identity unwittingly reproduces the worst features of Enlightenment philosophy (which supports liberal doctrine) and thereby loses sight of the countercultural, emancipatory spirit of lesbian scholarship, which Phelan would retain. Indeed, in its treatment of this disturbing tendency, *Identity Politics* resonates with Horkheimer and Adorno's *Dialectic of Enlightenment* (1972), a text aimed at demonstrating the totalitarian legacy of a putatively liberationist movement in philosophy.

Phelan fears that such a fate is in store for lesbian feminism, which has proved incapable of discrediting the liberal paradigm precisely because of this indebtedness to Enlightenment philosophy: the master's tools will never dismantle his house. Although her discussion of lesbianism's uneven relationship to feminism—couched in terms of Adrienne Rich's lesbian continuum and Gayle Rubin's sex radicalism—the pornography debates, and the factionalizing over sadomasochism implies that such an ending could never befall so divided and heterogeneous a group, Phelan demonstrates that infighting and factionalism among lesbians has not held theorizing about their identity in check.

Still, *Identity Politics* reveals the author's desire to see a communitarian ideology prevail over liberal individualism. Community provides standards and bestows on lesbians and other marginalized groups the recognition and dignity they deserve. Yet what kind of community does Phelan advocate? Lesbian separatism? Assimilation into heterosexual society? We have seen that her indebtedness to postmodernist theory makes trouble for the very concept of a lesbian identity. Hence, the author's desire for community is tempered by an awareness of its complications: she neither believes in pure identity nor wishes to compromise the needs of lesbians and saddle them with a false consciousness: "The false consciousness position . . . serves to bind people together by means of shared ideals and norms; the rejection of that position in favor of simple subjectivity rests on the refusal to truly enter a community that has limitation as its inevitable price" (p. 132). This statement summarizes the dilemma that *Identity Politics* seemingly confronts. The reader anticipates an impasse in the analysis, which, interestingly, Phelan avoids.

In fact, this problematic allows the most mature and subtle dimensions of Phelan's argument to emerge. Her unwillingness to capitulate to either false consciousness or totalizing theory inspires an insightful commentary on the need for a "situated self," one that assumes a political identity carrying no pretense of ontological foundation. Drawing on the works of modernist and postmodernist thinkers, Phelan demonstrates the urgency of adopting such a theory of the subject, one that recognizes the discursive yet political nature of any identity. Indeed, the author's analytic acumen is most striking in its resultant advocacy of lightheartedness and humor, "the humor that comes from seeing all categories, all explanations, all identities as provisional" (p. 156). This lightheartedness distinguishes Phelan's book from other pieces of feminist scholarship, which insist on lesbianism's subversive potential and cling to traditional theories of the subject. To Phelan, the political ineffectiveness of identity politics—which have not thwarted the liberal paradigm—hardly signal their failure: the opposite is true. She instead illustrates how contemporary theories of the subject have bearing on lesbian scholarship, an illustration that renders her own text invaluable.

MARY CAPUTI

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Liberalism and the Moral Life. Edited by Nancy L. Rosenblum. Cambridge: Harvard University Press, 1989. 302p. $32.50.

This collection of essays addresses two of the long-standing criticisms of liberalism as a doctrine of moral and political philosophy. The liberal injunction that government be neutral as to the ways of life that citizens might pursue has been attacked as an endorsement of, and an invitation to moral indifference. The liberal emphasis on the individual has been pilloried for separating people from each other in ways that are ontologically inaccurate and/or morally pernicious. Most (although not all) of the authors in this collection wish either to defend liberalism against these charges or to recast liberalism to make it more defensible against them. Taken as a whole, the essays reveal three different ways in which liberalism intersects with morality. First, it intersects at a level of the justification for liberalism itself. In "The Liberalism of Fear," Judith N. Shklar argues that both liberal neutrality and the liberal emphasis on individual rights are justified by their role in reducing the massive cruelty against people inflicted by governments that enforce their doctrine of the good life or that trample on individuals in pursuit of some overall social good. "Too great a part of past and present political experience is neglected when we ignore the annual reports of Amnesty International and of contemporary warfare," Shklar writes (p. 36).

Second, liberalism intersects with morality by setting limits for its own neutrality. This is the central theme in William Galston's essay, "Civic Education in the Liberal State." Because liberalism cannot be sustained if people are intolerant, he argues, liberalism cannot require neutrality between tolerance and intolerance. Nor can it be neutral to civic involvement and civic indifference, Galston argues, because civic involvement is required to put a check on overreaching by government officials.

Finally, liberalism offers its own ideal of the good life, namely, the ideal of individuality. This ideal is the subject of George Kateb's essay, "Democratic Individuality and the Meaning of Rights." Drawing mostly on Emerson and Thoreau (though he could have drawn on parts of J. S. Mill's *On Liberty* as well), Kateb places before us the ideal of the consciously self-chosen life, where "the regime of habit or custom or convention [is] shaken," where tolerance extends not merely to a grudging acceptance of the ways of life of others but "into an active generosity of perception, into a receptivity or responsiveness" toward new ideas or impulses (p. 189).

These three essays are the best of those that deal specifically with the criticisms of liberalism for moral indifference. They will make excellent readings in political philosophy courses. But overall, their level of philosophical depth disappoints. The notion that liberalism intersects with morality at the level of its own justification should hardly surprise. How else can a theory containing all sorts of moral words such as *should* be justified but by relation to a larger moral theory? Absent a moral theory, the liberal fear of cruelty and strife would occasion no reaction. As for the requirement that liberalism not be neutral between tolerance and intolerance, the argument is either less complicated or more complicated than Galston allows. Tolerance of others in the sense of accepting their choices about what kinds of lives to lead does not need to be justified as a requirement for liberalism to survive. It can be derived out of a moral duty to respect people (and hence part of the domain of the right—about which liberalism does not require government neutrality—rather than the good). But tolerance in the wider, Katebian sense of open-mindedness and questioning of tradition is, I would think, neither a prerequisite for the survival of liberalism nor a moral obligation. It is a vision of the good life that—if liberals are to advocate its cultivation—must be given more justification than Galston (or Kateb) give it. In addition, I would have liked to have seen these essays advance the discussion of these topics more than they do. For example, none of the discussions of the difficult and important issue of the connection (or lack of connection) between tolerance and moral skepticism come close to rivaling David Brink's brief discussion in his *Moral Realism and the Foundations of Ethics* (1989). And I would have loved to have seen some deeper treatment of the status of governmental endorsement or discouragement for ways of life that fall short of requirements or prohibitions.

The essays that discuss communitarianism and the criticism of liberalism for excessive individualism make more of an original contribution. In her essay, "Pluralism and Self-Defense," Nancy Rosenblum makes the telling point that contemporary communitarianism separates the call for community from traditional communitarian political movements, for which cohesion served a larger moral purpose than the enrichment of individual lives. "The contemporary communitarian purpose in opposing community to atomistic individualism is not the traditional one of transcending the self through identification with a group or nation," Rosenblum writes. "On the contrary, the object is to recover strong expressive selves, to make 'thin' selves 'thick' " (p. 218). But when the argument downplays the criticism of liberalism that atomism undercuts the inclination to behave morally, it downplays its own bite.

Stephen Holmes' essay, "The Permanent Structure of Antiliberal Thought," is extremely useful, despite a somewhat polemical tone. Holmes chides present-day communitarians for misinterpreting liberalism by yanking it from its historic roots. Communitarians, for example, contrast competition as a liberal ideal with brotherly love. But in the history of the development of the idea, Holmes notes, liberals contrasted competition not with brotherly love but with monopoly. Similarly, Holmes argues, communitarians contrast the maxim *I can do whatever I want* with the demands of morality; while liberals originally contrasted it instead with *I must do whatever my master or my social rank demands.* Holmes thus accuses communitarians of ignoring the contextualism they otherwise regard as so important.

STEVEN KELMAN

Harvard University

Feminism, Marriage and the Law in Victorian England, 1850–1895. By Mary Lyndon Shanley. Princeton: Princeton University Press, 1989. 213p. $25.00.

Contemporary feminists argue constantly about whether or not the women's movement should continue to couch its demands in the language of formal equality and universal rights. Radical feminists contend that to do so only obscures the social and economic differences that now exist between men and women and that if feminists do not place these differences at the center of their analyses, they will never be able to ensure the equal participation of men and women in the public sphere. Liberal feminists respond that formal equality and universal rights are still crucial to the empowerment of women in contemporary society and that if feminists place gender differences at the center of their analyses, they will facilitate the reintroduction of male supremacy in both law and public policy.

While members of both groups may want to come to terms with their differences, they are rarely able to do so—in large part as a result of their shared failure to grasp both the historical contexts (in which calls for equality and difference make political sense) and the extent to which liberal feminism might itself pose a radical challenge to liberal beliefs and institutions. Mary Lyndon Shanley helps them out of their dilemma in her extraordinarily insightful and beautifully written study of the rise of liberal feminism in England during the years 1850–1895 by focusing her attention on the particular historical controversies in which calls for difference came to be replaced by calls for equality. The result is an important piece of historical scholarship that should be of great use not only to specialists of the period but to all political philosophers and feminist theorists who are concerned with the value of liberal arguments to the achievement of gender equality in society.

Shanley sets herself two primary goals in *Feminism, Marriage, and the Law in Victorian England.* One is to explore the concrete historical contexts in which Victorian feminists rejected calls for difference and used liberal arguments about universal rights and formal equality to fight for legislative reforms in marriage and the family. The other is to underscore the various social and economic differences that existed between women and men during this period that were bound to keep women subordinated to men in both their private and public lives. By pursuing both of these goals together, Shanley manages not only to provide a lively account of Victorian feminism, but to articulate both the limitations and radical potential of the more abstract tenets of liberal feminist theory.

Shanley begins her study by arguing that while Victorian feminists such as Barbara Leigh Smith and Frances Power Cobbe saw legal reform as the key to women's emancipation, they did not, as is often assumed, appeal solely to suffrage or other public rights. Instead, they sought to transform the situation of women in the private sphere by replacing existing marriage and property laws—which, they argued, grossly violated a married woman's right to freedom and equality—with ones based on the gender-neutral values of mutuality and free consent. By opting for the language of gender equality in this context, they self-consciously committed themselves to liberal feminism. But, as Shanley clearly points out, they also challenged existing liberal beliefs and institutions by exposing the interlocking character of women's subjection in marriage and the state and by challenging the prevailing liberal assumption that the family and politics, the private and public spheres, are of a different moral order.

Since their starting points were liberal, rather than radical, we might expect them to have been moderately successful in their political endeavors. But they were not. While they won partial victories during the years 1850–1859, they were more often than not defeated by a Parliament whose members continued to view family relationships as outside the sphere of political justice. Moreover, even the battles they did win were often the result not of their own political agitation, but of various independent trends and attitudes, ranging from the rise of a middle class whose wealth was becoming steadily more mobile to the more paternalistic concerns of the state to protect lower-class women from the violence and brutality of their lower-class husbands.

How are we to assess the efforts of Victorian feminists such as Smith and Cobbe? Shanley points out that without the assertion of liberal values by the women's movement during this period later feminists might never have been able to talk about political justice in the family or call for the emancipation of women from patriarchal relationships. But clearly, these same liberal values (especially that of gender neutrality) held Victorian feminists back from getting at the social and economic conditions that kept women subordinated to men in both the private and public spheres—conditions that, Shanley argues, could have been overcome only with further analysis into the gender differences that Victorian feminists perceived as so threatening to their chances for achieving gender equality in both marriage and politics.

MARION SMILEY

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Freedom and the End of Reason: On the Moral Foundations of Kant's Critical Philosophy. By Richard L. Velkley. Chicago: University of Chicago Press, 1989. 222p. $29.95.

Henry Veatch once asked the question whether Kant was the *eminence gris* of contemporary ethical theory. The answer surely has to be an unqualified *yes.* From Rawls to Habermas—and encompassing a range of positions in between—moral and political philosophers have turned to the sage of Königsberg for their theories of equality, personality, and social justice. Kant's belief in the determinative power of human reason seems to many to provide a solid basis for establishing human rights and dignity in a world constituted by a gaggle of competing ideologies.

At the same time, Kant has been singled out as the philosopher most uniquely responsible for the failings of modern liberalism. For such thinkers as Iris Murdoch, Alasdair MacIntyre, and Michael Sandel, Kant's deontologically impoverished conception of the self leaves the individual dangerously cut off from the affective ties of time, history, and cultural tradition. Echoing Hegel's charge that Kantian ethics is an "empty formalism," these theorists all blame Kant for the moral vacuum currently said to occupy the center of our public life.

Between these two extremes a middle ground has slowly begun to emerge. Scholars like William Galston, Patrick Riley, and Yirmiyahu Yovel have all emphasized the teleological component of Kant's moral thought as a way of avoiding the charge of empty formalism but without sacrificing the metatheoretical underpinnings. Consequently, Kant's conception of culture in the *Third Critique* and moral teleology as a "fact of reason" in the *Second Critique* have become (in Riley's felicitous phrase) a way of putting "some non-heteronomous teleological flesh" upon "the bare bones of [Kantian] universality." From now on, the name of Richard L. Velkley will have to be considered a part of this distinguished company.

Velkley's ambitious work, as the subtitle indicates, is concerned with the *foundations* of Kant's critical philosophy. The term *foundations* is used here in the twofold sense of logical presuppositions and origins or beginnings (190, n. 1). He traces these origins back to the "precritical" writings of the 1760s (especially the so-called *Nachlass*), which locate the source of Kant's project in Rousseau. Velkley's concern with the Rousseauean inspiration of the critical philosophy is more than an antiquarian's attempt to identify the correct sources, or influences for Kant's doctrines. His claim is the stronger one that only after we see Kant's mature thought as being decisively shaped by such typically Rousseauean themes as the self-alienation of reason, the paradoxes of progress or perfectibility, the power of culture (*Bildung)* to redeem humanity, and the striving for totality will we be in a position to understand Kant's problematic relation to modernity as a whole (pp. 3–4, 41–42, 165, and 183–84, n. 71). The picture of Kant that emerges from this fascinating study is at once richer and more complex than the straw man who often appears in the works of defenders and detractors alike.

A not-insignificant aspect of this book is the light that it sheds on the themes of reason and happiness and the dialectical relationship between them. Rather than presenting Kant as the philosopher's philosopher responding to a set of traditional epistemological quandaries, it depicts him as seeking a rational foundation for the dignity of mankind that he claimed to have discovered in his critical confrontation with Rousseau's *Emile* (pp. 37, 49-52, 61-88). The Kantian emphasis on the primacy of practical reason represented Kant's attempt to offset the Enlightenment's reduction of reason to an instrument of the desires or a slave to the passions (in Hume's famous phrase). The primacy thesis was Kant's Herculean effort (see the delightful picture on the jacket cover) to vindicate the sublimity of moral experience by radically distinguishing it from both naturalistic schemes and the traditional claims of revelation. No doubt purists will resist Velkley's controversial claim that the *First Critique* was but a propaedeutic to Kant's practical philosophy and the creation of a realm of self-legislating moral reason (p. 188, n. 26).

There are two further dimensions of Kant's thought that Velkley's study will help to illuminate. First, it will lend greater precision to the claims of earlier Kant scholars like Cassirer and Beck, who interpreted Kant as a deepened or radicalized Rousseau. Second, and more importantly, Velkley shows that the idea of culture was Kant's means of supplying some substantive content to the doctrine of self-legislative reason. The concept of a culture of reason—once again borrowed from Rousseau—provided Kant with his understanding of the highest good or the true vocation of man (pp. 10-11, 156). While our free rationality is the basis of culture, culture also modifies and makes possible the very exercise of reason. The task of the critical philosophy is to explore the mutually determining relation between reason and culture in all its depth and scope.

Finally, not the least of the merits of this fine work is that it underscores the need for a critical English translation of Kant's *Nachlass.*

STEVEN B. SMITH

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The Presence of the Past: Essays on the State and the Constitution. By Sheldon S. Wolin. Baltimore: Johns Hopkins University Press, 1989. 204p. $29.95.

In this book, our foremost theorist of the political moves beyond his classical work to a critique of the state. Sheldon Wolin's *Politics and Vision* set forth a concept of the political as a sphere in which we deliberate and act on our common concerns. His present work retains the notion of deliberation but now stresses deliberation about our differences. The concept of power provides a bridge between his early and later works; influenced by Michel Foucault and others, he now conceives power as more decentered yet more total in its reach than it was before. Meaningful politics is "public discourse over how power is to be used" (p. 182).

The Presence of the Past is a collection of essays written separately but with recurring themes. In the years preceding their composition Wolin edited *Democracy,* a journal devoted to the possibilities of intellectually informed democratic citizenship. Departing from classical theory, he sees democracy—the initiating of cooperative action and accumulation of political experience by all people—as the best political form. Civic republicanism is criticized (in practice if not as an idea); nor is the "rule of a republican elite with democratic consent" (p. 48) the same as democracy, as "a political life-form" (p. 196).

Wolin argues that the state is inherently antidemocratic, and the word *government* deceptive, obscuring a new megastate. Under President Reagan, "the state [was] slightly decentered but not significantly decentralized" (p. 174). The state expands power and mixes the public and private spheres (in war and space technology), penetrating into social life (in drug testing). Democracy currently serves only to legitimate the state. True democracy can only occur on a small scale, with shared power. Wolin finds a connection between a "warfare state" and the welfare state, with origins in Hamilton's new "science of politics" at the American founding (p. 111). He does not do justice to democratic reasons why we might have wanted a strong state at crucial junctures in our history.

We might consider his various discussions of power on four levels. First, power exists at the surface level in institutions, such as the political executive and the national bureaucracies. (There is little mention of legislatures, which might alter some of the book's conclusions.) The state is a "strong but heteronomous state" (p. 21), interdependent with corporate power. Second, institutions are legitimated by modes of discourse, such as the idea of the "economic polity" (pp. 41–45, 147–49). Noneconomic issues vanish from the political agenda. "The narrative structure of justice . . . is denied" (p. 45), and interpretive modes of discourse are devalued in education and public life.

These institutional structures and modes of discourse inhibit modes of action. Bureaucratic rationality, pursuing the goal of "rational productivity" (p. 171), posits certain "necessities" that constrain action. It controls members of the underclass by creating the "dangling man" who survives at the whim of the bureaucracy's changing rules (p. 163). What had previously been a doctrine of state action only in times of severe threat, usually external, becomes, in post-Machiavellian times, a theory to justify the continuous incursion of the state into everyday life. National "political necessity" expands, hurting the political cultures of the different states.

Power has institutional, discursive, and behavioral forms; it also issues in forms of identity. Yet Wolin hopes that the U.S. public can resist the growth of a mentality in which science and rationality are equated. U.S. identity may lie "essentially in its contested character" (p. 10). Politics and local political action are our birthright, deeper than contract (pp. 137–38). We can counter the forms of state power by celebrating our differences. Here he is influenced by recent feminist and postmodern accounts. He embraces a "political polytheism" (p. 124). The abandonment of a political monotheism raises, of course, many questions.

The best essays in the book are those analyzing a text in political theory, such as the Old Testament, Locke, Tocqueville, and Montesquieu. Here Wolin is graceful and illuminates old texts with new questions. The analyses of Tocqueville on time and municipal spirit and Montesquieu on intermediary bodies as the "domestication of power by an unplotted conspiracy of difference" (p. 107) are brilliant. The least successful essays seem to be those on American political culture; less convincing assertions include that political parties are typically highly bureaucratized (p. 139), that education and sexual reproduction used to be considered private matters (p. 172), and that higher education in state universities and colleges is the last, best hope for democratic values (p. 48). More engaging are the conceptual essays, which, while perhaps overly populated with dichotomies, still teach new things through their contrasts. And here the subtext is, How can we as intellectuals relate to power and democracy?

It is not clear what grounds there are to stand on. The book has an appeal to a past; but that past cannot be allowed either to continue privilege (which would violate the premise of equality) or to include ethnic roots (which he disparages as hopelessly gone). One might then ask, Which differences are valuable? Which are not? What is justice? Are we to judge? For Wolin, the past must live through a memory of past injustices and the experience of political action. He quotes Richard Hooker to the effect that five hundred years of acts are ours. In defending the value of political experience, Wolin stays true to his distinctive ground.

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Natural Right and the American Imagination. By Catherine H. Zuckert. Savage, MD: Rowman Littlefield, 1990. 284p. $33.50.

Catherine H. Zuckert's *Natural Right and the American Imagination* is a remarkable book. By examining the works of American novelists, Zuckert uncovers a tradition of American political thought of which political theorists have been largely unaware. American political theorists typically turn to the writings of statesmen, such as the *Federalist, Notes on Virginia,* and *Congressional Government.* Zuckert points out that such works were written by active statesmen facing practical problems. The novelists she treats, in contrast, deal with the most fundamental questions of political theory—the relation between nature and political life, the foundation of community, the issue of human happiness. They do so, moreover, through the peculiarly American theme of a return to nature. The recurrence of this theme in American literature is due not to youthful escapism or the frontier experience, as literary critics maintain, but to philosophic reflection on the founding principles of the Declaration of Independence.

Commonplace interpretations of the American polity are Lockean: community is based on agreement that secures life and economic advantage. But just as Rousseau and later philosophers criticized Locke and the liberal regimes that grew out of his principles, American novelists implicitly criticized the Lockean understanding of community. They did not merely reflect on American principles as embodied in their stories, Zuckert argues. They consciously reinterpreted those principles in the process. Although scholars are sometimes guilty of imposing philosophic frameworks on literary texts, Zuckert provides ample evidence and argument for her thesis. Cooper himself states that his novels are meant to illustrate the true meaning of American political principles. Through his *Leatherstocking Tales,* he urges that the natural goodness of his backwoodsman hero Natty Bumppo, his compassion, his modesty before creation, and his respect for the integrity of others be incorporated into the laws of the community as standards of right. While other novelists diverge from Cooper's understanding of nature's goodness, they follow him in searching for a non-Lockean basis for community. For Hawthorne, support and sympathy for humanity's turbulent inner life can be found in the family—and the polity that protects such unions. For Melville, community must be based on the sympathy arising from common human suffering and mortality, as well as a shared appreciation for the spiritual courage necessary to face cosmic indifference.

Twain and Hemingway are less sanguine about community. In *Huckleberry Finn,* Twain criticizes the American tendency to seek escape from the restraints of law and society to a romanticized version of nature. Hemingway's hero Nick Adams finds solace only when alone in nature, experiencing fleeting sensations of life's goodness. It is Faulkner who provides the most enduring basis for community. The events of the past must be pieced together, interpreted in light of the present and aspirations for the future. Acknowledging his debt to Bergson's theories of time, Faulkner teaches that communities must be based on such interpretations and reinterpretations. These interpretations, which give meaning to suffering and injustice, reveal the common passions, fears, hopes, and aspirations, so that people can embrace one another "in humility and pity and sufferance and pride."

These literary interpretations of nature and society, Zuckert argues, have become part of the American *polity*—understood not narrowly as the institutions and processes of government but as the opinions and mores of the people that give those institutions and processes meaning. Much as the past according to Faulkner can and must be interpreted by the present, so is the *Declaration* flexible enough to permit, and incomplete enough to require interpretation. Because of its practical purpose, the *Declaration* leaves many questions about the meaning of natural right and civil society unanswered. By offering answers to these questions, American novelists participate in an ongoing founding of their political community. They engage in statesmanship.

Zuckert shows, subtly and persuasively, how the themes of American literature resonate with those of modern thought, especially in its criticism of classical liberalism. She explains how American novelists have selected what is best in Rousseau, turned to what is sound in Bergson, and avoided the antidemocratic and nihilistic extremes of Nietzsche and Heidegger. Their works have served as vehicles for incorporating philosophic thought into the American polity. The American mind is therefore not closed to philosophy—at least to a philosophy that questions traditional principles in order to strengthen them, alienating citizens neither from their tradition nor from political life itself. By interpreting these American novels as expressions of self-conscious statesmanship, Zuckert has imitated their authors, who reshaped America's self-understanding. Zuckert brings us to the point where philosophy and politics intersect. Few projects have such depth.

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AMERICAN POLITICS

Political Culture and Public Opinion. Edited by Arthur Asa Berger. New Brunswick, NJ: Transaction, 1989. 194p. $16.95 paper.

Manipulating Public Opinion: Essays on Public Opinion As a Dependent Variable. Edited by Michael Margolis and Gary A. Mauser. Pacific Grove, CA: Brooks/Cole, 1989. 423p. $23.75 paper.

The paradox of the study of public opinion is that as our tools for measuring it have grown ever more sophisticated, our understanding of the basic concept has shown far less, if any improvement. Despite thousands of surveys telling us what the public thinks on issues ranging from nuclear war to the marital woes of Donald Trump, scholars cannot even agree on a definition of *public opinion.* As Elisabeth Noelle-Neumann has written in *The Spiral of Silence* (1984), "Generations of philosophers, jurists, historians, political theorists, and journalism scholars have torn their hair in the attempt to provide a clear definition of public opinion" with little success.

Although neither of these books provides the missing definition, each suggests an interesting approach to understanding a more manageable question that all students of public opinion have tried to answer—what factor or factors most influence public opinion. According to Arthur Asa Berger, culture, while not the primary influence on public opinion, can be "a means of supplementing the approaches we have developed to help us understand politics" (p. 4). Instead of precisely defining either *public opinion* or *political culture,* he tells the reader that the meaning of the former is "rather obvious" and that the latter, although "a bit more difficult to define" can be understood by listing some of the definitions of major scholars (pp. 1–3). This failure to grapple with difficult conceptual issues is symptomatic of the book's weaknesses.

The key essay is Aaron Wildavsky's 1986 APSA presidential address, in which he argues that self-interest is primarily developed by people seeking to support their way of life or culture. For Wildavsky four such cultures—hierarchical collectivism, egalitarianism, competitive individualism, and fatalism—define people's identities and allow them "to know what they prefer without knowing much else" (p. 28). He suggests that his cultural theory provides a better understanding of the dynamics of public opinion than such commonly used distinctions as Left-Right or liberal-conservative.

Since the remaining chapters are divided into Wildavsky's four cultures, a reader would expect either a further development of these concepts or a test of their explanatory power; but none of these essays, except for Wildavsky's review of the film *Thief* (which briefly elaborates on his views of individualism and collectivism) even mentions his categories. Several make little or no mention of culture at all; and those that do mention it use it not in Wildavsky's sense but in the more conventional meaning of national identity. The volume includes essays on new information technologies, television in Brazil, feminist challenges to Federal Communications Commission broadcast licenses, the Vietnam Veterans Memorial, graffiti, and the Macintosh commercial broadcast during the 1984 Superbowl. It is hard to tell what kind of audience would be interested in such a range of articles introducing topics of widely varying importance. Aside from Wildavsky's essay, one of the few others that seek to address larger issues is Leo Bogart's "Mass Communication and World Opinion." Bogart argues that we cannot speak about "world public opinion" despite the possibility of taking worldwide surveys because national differences give even identical questions different meanings in different countries. Like Wildavsky, Bogart suggests some interesting possibilities for future research to evaluate the possibilities for more common values among now diverse cultures. There is a real need for more work on political culture and public opinion and either Bogart's or Wildavsky's ideas would provide a good starting point.

Manipulating Public Opinion is a far more focused collection. Its editors argue that the development of polling has led much of public opinion research to assume that mass opinion drives policy. Instead, they suggest that surveys can be better viewed as showing how successfully elites gain popular acceptance of their preferred positions. "The authors of these essays consider the extent to which public opinion in diverse circumstances is subject to manipulation by political elites" (p. 3). The collection is divided into three sections—electoral politics, public policy, and long-term influences—after which Margolis and Mauser provide an overview evaluating the effectiveness and limits of elite influence on public opinion.

Although not as original as the editors imply, the essays in the first two sections provide an excellent review of recent research and debates in these areas. Xandra Kayden bolsters the argument that many scholars are now making, namely, that the national party organizations are growing stronger, by examining changes in the Republican National Committee. David Magleby writes about referenda, a topic that until recently has been neglected by researchers, who preferred studying candidate contests. Mauser's discussion of political campaigns' use of market research techniques, especially multivariate techniques that map voter preferences, is hampered by the fact that all the candidates he discusses lost badly. Since these techniques have been used far more successfully by Republican presidential candidates. a look at one of their campaigns would have made the importance of marketing strategies clearer. The essays in the second part provide similar reviews and overviews of the use of the media in shaping opinion and policy, especially on the subject of agenda setting.

However, the most important essays are in the third part. The first two sections dealt with all influences on public opinion; only in this section is the separate idea of elite manipulation of mass opinion developed. Benjamin Ginsberg argues that polling has served to aid governments in managing public opinion, an argument he has developed in more detail in *The Captive Public* (1986). Benjamin Page and Robert Shapiro examine the factors that influence public opinion and the extent to which they can be manipulated. W. Lance Bennett adds a case study of public opinion on U.S. policy toward Nicaragua which shows both the success and limits of manipulation of public opinion. A single case study, especially of a foreign policy issue, is limited in its definitiveness; but Bennett's conclusions are quite provocative.

Overall, both old hands and those new to the field will find *Manipulating Public Opinion* a useful and stimulating collection. Despite an occasional tendency to overstate the importance of their findings, most of the authors have contributed articles that will make us think.

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Biomedical Technology and Public Policy. Edited by Robert H. Blank and Miriam K. Mills. New York: Greenwood, 1989. 235p. $42.95.

Dangerous Diagnostics: The Social Power of Biological Information. By Dorothy Nelkin and Laurence Tancredi. New York: Basic Books, 1989. 207p. $18.95.

When Technology Wounds: The Human Consequences of Progress. By Chellis Glendinning. New York: William Morrow, 1990. 285p. $18.95.

It is by now a cliché that we are better at creating policy responses to new technologies after their darker sides are clear than at anticipating problems. The three books reviewed here shed light on different aspects of this cliché: as its name suggests, *When Technology Wounds* attempts to document harms imposed by various technologies; while the other two books seek to help us recognize problems before it is too late. None of them provides more than a cursory set of policy recommendations, however, further confirming the cliché.

When Technology Wounds recounts the stories of 46 "technology survivors"—people who have been harmed by technologies in the workplace and environment. Several of them sustained long-term injuries from exposure to new medical technologies, especially reproductive technologies such as the Dalkon Shield or DES. Reproductive technologies also comprise 5 of the 11 technologies considered in *Biomedical Technology and Public Policy.* Not only has there been rapid change in reproductive technologies, but their very intimacy heightens fears of them at the same time as it raises serious questions about the proper role of government in regulating them. Other chapters consider other issues of treatment, (such as organ transplant) and overuse of technology at the end of life. *Dangerous Diagnostics* raises similar issues but in the context of our rapidly expanding abilities to diagnose, or at least identify, factors that may predispose individuals to future disease or problems.

An important thread that runs through the latter two books is concern for how technologies are used to maintain or strengthen social institutions. The medicalization of reproduction and childbirth (as shown, for example, by the increasing use of fetal monitors, which were initially intended only for babies at high risk) is a logical response to the tort liability system but also reinforces it. At the same time, it turns women back into objects, or cases. This trend is especially important for new diagnostic tests, the results of which may allow employers to bar forever mere carriers of genes for degenerative diseases from responsible new positions or allow schools to consign minority children to permanent remedial classes on the basis of a single test in kindergarten. As might be expected from this discussion, several of the writers focus on the civil rights implications of the technologies.

A second theme of both books is the cost of medical care. As new technologies become available, U.S. consumers have a tendency to want to use them. Resource constraints have already begun to force us to choose among treatments and diseases; the state of Oregon recently asked its residents to help decide which medical procedures were most important and cost-effective in order to ration limited medical assistance funds. Other governmental agencies will also have to bite the bullet in confronting trade-offs among medical technologies. Again, these decisions are complicated by our legal system: doctors do not always feel free not to use the best available treatment or diagnostics lest they be sued later.

Despite their underlying similarities, the two books differ in important respects. The authors of *Dangerous Diagnostics*, Nelkin and Tancredi, consider only one category of technology. This allows them to deploy Tancredi's medical training to good effect in providing relatively extended descriptions of the technologies of diagnosis—their limitations and theoretical shortcomings as well as their strengths. The writers in *Biomedical Technology* consider more familiar technologies and provide us with fewer descriptions of them. On the other hand, Nelkin and Tancredi have written for a broader audience, providing a more gee-whiz treatment than the sober but generally well written essays in Blank and Mills, many of which were included in a symposium published in the *Policy Studies Journal*. This is a coherent collection of related essays.

While both books do an excellent job of pointing to the deep policy implications of new medical technologies, neither is very strong on policy solutions. Several of the authors imply or state explicitly that when technology is changing so fast it is difficult to develop useful policies general enough to hold up for reasonable periods of time. Moreover, until the technologies are in place, it is often difficult even to formulate the right policy questions. For example, Susan Behuniak-Long's article in *Biomedical Technology* shows how the Supreme Court decision in *Roe* v. *Wade* is being extended to new reproductive technologies such as in vitro fertilization, for which it was obviously never intended. In light of these new technologies, however, we can now see how flawed the definitions of *birth* and *person* are in that famous decision. The questions asked in 1972 reflected the limited technology of the times and could not (or at least did not) anticipate new technological developments.

However difficult it is to formulate new policies, it is nevertheless the obligation of the policy analyst to assist. If these writers, familiar as they are with the legal, social, and technical bases of the problems raised by the new technologies, do not offer specific suggestions for solutions, what basis will the rest of us have to seek action in the public sector? *When Technology Wounds*, otherwise a superficial and emotional recounting of the stories of a nonrandom sample of people who have sustained long-term injuries from new technologies, does at least try to offer some solutions. Glendinning proposes a new interest group, a Union of Technology Survivors, who can organize the educational, legal, and political resources needed to further their policy agenda—a combination of support payments and technology controls. She stresses the importance of collective action and of bringing together the creators and the users of technologies. Charming and naive, perhaps—but also sensitive to the difficulties of acting without government. Nelkin and Tancredi also briefly direct our attention to government as the only possible agent for protecting individual rights threatened by diagnostic technologies. Their recommendations are on the order of *Policies should be developed* rather than specific suggestions for action.

In their diverse ways, each of the three books is stimulating. All might find their uses in the classroom. Glendinning's book would require considerable professional oversight to ensure that students were aware of its shortcomings, but either of the other two would be appropriate for courses in science policy, public policy, health policy, and legal or civil rights. Upper-level undergraduates should be quite able to read even *Biomedical Technology*, whose essays are more dense and scholarly than the other two works. The issues raised by all three, especially *Dangerous Diagnostics* and *Biomedical Technology*, will be with us for many years to come. Opening the eyes of a new generation to the problems of technologies they often accept without question is an important contribution to our future well-being.

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The Constitution and the Regulation of Society. Edited by Gary C. Bryner and Dennis L. Thompson. Brigham Young University, 1988. 258p. $34.50 cloth, $10.95 paper.

Occasionally there comes along a book, read for some instrumental purpose, that reminds me why I entered an academic field. My field is regulation and bureaucracy. The wonderful collection of very different essays, *The Constitution and the Regulation of Society*, reveals how engaging intelligent thinking about the fourth branch of government and its constitutional underpinnings can be.

The eight essays in this edited volume and the five sections of generally lively discussion interjected between the chapters range from critiques of modern risk assessment used in the regulatory process to eloquent arguments that the bureaucrat should be motivated by love and that the Constitution assumes this. Leading scholars in the field of regulation, approaching that potentially deadly subject from so many different routes, are brought together in one volume. The work, while it may be disassembled for a graduate course in regulation and society, is perhaps best read next to a fire with a glass of sherry in hand. Or the volume may serve as a reader for an advanced tutorial in political science, administration, or a related course.

The interchapter discussions—occasionally a bit off-putting, since the authors sound as if they, in dialogue, are even more articulate than the written words of Locke and Plato that they quote so freely—are generally very rich. As one author notes, these are "intelligent, lucid and well-moderated papers" that address *"quite different versions of the good life"* (p. 172; emphasis original). They also take on fundamental questions: (1) In what matters should the state involve itself? and (2)—even more basic—Is it possible to arrive at "moral truths that are superior" and if so, how) (p. 175).

Despite the diversity of approaches, this book has a major and important theme. The editors state it succinctly: "Regulation poses challenges to the American political system that go beyond current economic and political debate and touch its constitutional underpinnings" (p. 2). This theme is intertwined in a sufficient percentage of the work to give it coherence. Especially useful on the titled topic are the chapters by Wolin, Gawthrop, and Shapiro.

Beyond attention to this theme through little-known historical treatment, fairly esoteric philosophical analysis, and pragmatic political consideration, the individual chapters make their own contributions in predictable ways, given the excellence of the scholars assembled.

The lead-off Lowi piece is interesting for characterizing what is liberal and focusing on the individual in the conservative-liberal debate. He also uses analyses of a regulation binge and a welfare binge to set the stage for the debate; one might question whether his evidence is compelling; but his approach both in the essay and in his refreshingly argumentative comments in the dialogues is provocative both in his return to an earlier contribution of "interest group liberalism" and his new work on *regulation management.* This term is used to describe the type of deregulation of the Reagan era "not by terminating or shrinking any of the authority now held by the executive branch but by retaining the power and using it to control the agencies so as to reduce or delay the output of rules" (p. 28). Lowi weaves throughout his essay distinctions that will challenge traditional assumptions about who wishes to regulate, at what level of government, and for what purposes.

The Wildavsky chapter, "The Secret of Safety Lies in Danger," repeats a theme that he has expressed well elsewhere, namely, that a rapidly progressing society will usually be safer than a static one. The link to constitutionalism is strained, but the link to modern risk assessment in the regulatory process is direct and challenging: "substantial regulatory resources are being expended to reduce risks that are already trivial, while the public's exposure to the carcinogens that matter is being prolonged" (p. 60).

A. Don Sorenson in a long, complex essay attempts to start from basic principles and work to an analysis of government and regulation. Whether he reaches his objectives is difficult to say if one does not accept certain moral assumptions regarding human life, abortion, pornography. The thesis nonetheless is well placed in this volume: the question of regulation is raised as "concerning the relation between law and morality of human society," a theme that several of the authors address at one level of traditional spirituality or another. The questions he raises are gigantic; and one reading of the essay promotes an objective inquiry into regulation, morality and the constitution. But occasionally a position leaps out that makes a neutral reading and application impossible: "It would be a serious abuse of governmental power if it were used to protect or foster large-scale, indiscriminate destruction of unborn human life or to impoverish the deep-seated caring-for-life conception on which procreation is grounded" (p. 90).

There is also an insightful chapter by Sheldon Wolin addressing the fundamental question of power in, among other societies, a constitutional democracy (ours) where "most of the symbols, myths, and memories that compose the meaning of collective identity are meaningless" because of the "influx of potential citizens and resident aliens" (p. 102).

Owen Fiss instructs on the new procedure by providing a look back at structural reform litigation. His evaluation is tied to a careful presentation of the characteristics of the bureaucratic state. Most challenging is his analysis of the criteria according to which one should evaluate the successes of the new procedure. The defense of major institutional litigation is strong and will add to the debate on the value of major consent decrees and the function of special masters and powerful federal judges in controlling bureaucratic outcome. Fiss also offers a position on the new procedure that may be incontrovertible: "The authority of the judiciary to do what it is doing in schools or prisons does not turn on the success of its intervention, but rather on the quality of the process upon which the interaction is based, on the willingness of the judiciary both to engage in a dialogue on the meaning of public values and to remain independent from the parties and the body politic" (p. 138).

Nice companions to the Fiss chapter are those of Bruce C. Hafen and Martin Shapiro. Hafen's contribution on the regulation of "mediating structures" such as families, churches, neighborhoods and voluntary associations is well argued, impressively contextualized in the analyses of civil rights legislation and litigation, and in places eloquent. (Again, the initial premises may not be accepted.) Hafen offers a pattern for understanding "certain unique private American institutions which help individuals find and nurture personal meaning and value in their lives" (p. 148). Like Fiss, I would argue that the megastructures that Hafen postulates as countering the values of the mediating structures "are value-maintaining and value-generating as well." Martin Shapiro adds an analysis arguing that courts have made themselves the victims of their own rules by pushing for ultimately "right outcomes" but not having the expertise to judge the outcomes. (The chapter also offers one of the most lucid analyses of the evolution of administrative agencies that I have seen.)

Louis C. Gawthrop adds a historical perspective that is often lacking in the analysis of the development of the bureaucracy in the United States. His focus is on "the linkage between policy and administration as it has developed in the formal context of our constitutional democracy" (p. 182). Just as important, ranging from the Greeks and the Bible through the administrations of our most undistinguished presidents, the chapter lays out the sources of executive and bureaucratic authority in the Constitution and in the nineteenth-century political debates among the federalists and the republicans and their hybrid representatives. The historical insights put into a fuller perspective the debates on the growth of an impersonal undemocratic bureaucracy.

Overall, the volume is a satisfying treatment of a difficult subject. In the discussion, Wildavsky addresses accurately the challenge of attempts at consensus making on the proper and constitutional role of regulation: "The criteria for democracy are certainly being redefined. If one part of our people thinks you have to have greater equality of condition before you can legitimate institutions as democratic and the other thinks that democracy is what people can agree on after following certain procedures, it will not be easy to reconcile these views" (p. 173).

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Selling Hope: State Lotteries in America. By Charles T. Clotfelter and Philip J. Cook. Cambridge: Harvard University Press, 1989. 323p. $29.50.

Lotteries. By Alan J. Karcher. New Brunswick: Transaction, 1989. 118p. $24.95.

The United States is being swept by a third wave of legalized gambling. In the two very different but highly complementary books reviewed here, the fastest growing of those games, lotteries, is examined from the interdisciplinary perspectives of history, economics, sociology, psychology, and political science. Although this third wave can be traced to the era of Prohibition and the legalization of casinos in Nevada in 1931, the real beginnings of the modern-day lottery can be found in the institution in the early 1960s of the New Hampshire lottery. In the last three decades, religious objections and the fear of gambling's harmful consequences have not served to dampen the public's appetite for lotteries. Whether as a direct response to "the policies of Reaganomics" and the New Federalism, which required states to seek new sources of revenue (Karcher, p. 20) or the decline of "traditional American values" and the concomitant rise of "materialistic values" (Clotfelter and Cook, pp. 43, 45), by the late 1980s lotteries were legal in thirty-two states and the District of Columbia, an area encompassing three-fourths of the U.S. population.

Clotfelter and Cook have written what well may be the most comprehensive, balanced treatment of lotteries to date. Citing economic, sociological and political data, they have produced a first-rate book on a topic of growing concern to public policy makers in the latter twentieth century. Not content to rely on merely anecdotal evidence, Clotfelter and Cook have not only synthesized and summarized the empirical work of others but have themselves analyzed heretofore unaggregated data to present a wonderfully exhaustive picture of U.S. lotteries.

Unlike the social science tone taken by Clotfelter and Cook, Alan J. Karcher has written what is best described as a polemic on the pitfalls of lotteries from the perspective of one who, as a former state legislator and Speaker of the New Jersey Assembly, has been there. Although there is—and by necessity must be—some overlap between the contents of the two books, they are most fruitfully read as complementary works on the nature of U.S. lotteries. For instance, whereas Clotfelter and Cook painstakingly cite empirical sources and provide extensive footnotes to support their research, Karcher in his polemical style often simply assumes these as factual premises on which to base his recommendations to policy makers struggling with the issues of lotteries.

As noted, however, there is much which the two books have in common. Both review the historical development of lotteries throughout the world as well as the United States. Both examine the arguments for and against lotteries, including what is perhaps the strongest secular argument against them, the regressive nature of the implicit tax paid by lottery participants. Likewise, both examine the unique nature of lotteries in the context of government as both provider and regulator of lotteries; in the former, government's role must be to sell as many lottery tickets to as many people as possible and in the latter, to protect the polity from misleading advertising, play by minors and compulsive gambling. And both analyze the political realities of lottery adoption including the pressures brought to bear on state officials by supporters in the public, the lottery supply industry, and retailers who expect to realize lottery commissions.

Although Karcher has the advantage of having been an elected official who knows only too well the political pressures on state officials to implement and maintain a lottery, Clotfelter and Cook clearly have the decidedly superior presentation and analysis of these empirical data. From basic descriptive data to complex multivariate regression and probit analysis, they provide an amazingly complete and accurate view of the lottery industry. We find, for example, that only 50% of lottery funds is returned to the public in prizes and that whereas approximately one-half of lottery state revenues is derived from income and sales taxes, only three percent is a result of lottery revenues. Indeed, the authors found that in only four of the fifteen states they studied for 1986 would the sales tax have to increase more than 1% to raise the same amount of revenue as the highly visible lottery.

In answering questions about who plays the lottery and why, the authors devise a four-part typology of players: investors (who know the probabilities yet see lotteries as a type of risky investment), plungers (who see the lottery as their only hope for social status or financial security), believers (who seek out dream books and lottery advisors to guide them to financial reward), and participants (who play the lottery for the excitement, social interaction, and fame that it can bring). As one might expect, not all lottery players participate to the same degree. Not unlike alcohol sales, the approximately one-half of the population who play lotteries are not responsible for their ultimate success. About 10% of those who play account for 50% of the total amount wagered, and 20% of those who play account for 65% of the total wagered in a lottery. Consequently, unlike liquor, where excessive consumption is disparaged by government, an important component of the lottery business is to maintain the interest of those heavy players. Through targeted marketing and the introduction of new games such as lotto, the lotteries have managed to increase their appeal to those players. Those heavy players, according to the data, tend most often to be blacks in the East and Hispanics in the West, Catholics rather than Protestants, middle-aged rather than young or old. Particularly important, note Clotfelter and Cook, "the relatively poor spend a much larger fraction of their income on lottery tickets than the relatively affluent" (p. 241).

All lotteries in the United States are variations of what Clotfelter and Cook refer to as the Revenue Model. That model has as its objective "to generate as much revenue as possible for the state" (pp. 241–42). Yet, as they note, lotteries have the simultaneous effect of decreasing sales taxes to the extent that money used for lottery tickets is not purchasing consumer goods and may, in the long term, reduce productivity and the work ethic with its promise of quick riches. Yet the Revenue Model is only one form lotteries could take. The Consumer Model, the goal of which would be to serve the interests of the consumer, would require truthful advertising about the odds of winning and would have a higher payout rate on prizes. The Sumptuary Model, which would legalize lotteries but not encourage them, would have a low payout rate, with limited product variety and low-key advertising emphasizing informational messages only.

To the extent that the Clotfelter and Cook work has a weakness, it is in the lack of specific policy recommendations (which may, in part, be explained by the charter of the National Bureau of Economic Research under whose aegis they are publishing, which requires work to be "scientific" and "impartial"). It is in the realm of recommendations to policy makers that Karcher's work is most appealing, however. Lotteries are here to stay, he notes; for not only is the public addicted to them, but states dependent upon lottery revenues are loathe to end them. Consequently, he proposes not the elimination of lotteries but the implementation of what he sees as necessary reforms to ameliorate the social cost of lotteries—"a cost that is becoming increasingly heavier when measured in terms of existing abuses, potential exploitation that must come with a system driven by the imperative for increased revenue, and risk of budgetary disorder inherent in the advance appropriation of speculative income" (p. 113). Karcher's five policy recommendations would transform state lotteries from the Revenue Model (which they currently reflect) to something more akin to Clotfelter and Cook's Consumer Model, with higher payout rates, truth in advertising, and greater regulation over their more mercenary aspects.

As these two works illustrate, the study of lotteries is important to political and other social scientists not only for what these enterprises tell us about risk-taking behavior but also because of the policy choices and consequences awaiting the states that have adopted, or are considering adopting, a lottery.

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Making Laws and Making News: Media Strategies in the U.S. House of Representatives. By Timothy E. Cook. Washington: Brookings Institution, 1989. 210p. $26.95.

It will come as no surprise to observers of the Washington scene to find that the mass media have become increasingly prominent in congressional affairs. Legislators have learned quickly to take advantage of the access to print and visual media that their status affords them. Nor is it startling to hear that members of Congress and journalists (broadly defined) have a symbiotic relationship. As Timothy Cook puts it, in *Making Laws and Making News:* "Each side holds important power—members of the House by controlling whether, when, where, and how to grant access, journalists by deciding whether, when, where, and how to pay attention. The movements in the tango of their relationships are constantly subject to negotiation and renegotiation" (p. 31). These conventional wisdoms date from Robert O. Blanchard's 1974 anthology, *Congress and the News Media* and have pervaded the subsequent literature, which Cook cites extensively.

Cook's contribution—an important one—is to embellish and elaborate these themes. Drawing on personal observation, analysis of House members' visibility in the *New York Times* and evening television network news broadcasts, semistructured interviews with 40 House press secretaries, and a mail questionnaire sent to all press secretaries of Democratic members of the Ninety-eighth Congress, Cook clarifies the links between the members and the media. The central strand of the argument stresses the supplementary and complementary connections of inside, conventional politics and policy making, and outside, media-based strategies.

Using the media skillfully enables the members of Congress to secure reelection and thus to position themselves to advance their policy goals and careers in the legislature. Media attention is not an unmixed blessing, however; too much coverage may make the race too visible, to the benefit of less well known challengers. Adroit use of the perquisites of office—the frank, the newsletter, audio and video capabilities, and so on—enable the incumbents to make certain that the local media's limited coverage of the contest is "on their own terms" (p. 116).

Similarly, media strategies help members to get issues of concern on the political agenda, but their ability to do so is constrained by intra-House considerations. Unless the representatives are experts ("authoritative sources") and/or hold leadership positions in the party or on committees, the media may ignore them. Members will focus on issues to which their expertise applies and that satisfy "journalistic criteria of importance and interest" (p. 148). The combination of media needs and members' strategic locations in the legislature provides the basis for attracting coverage and raising an issue to public awareness.

Finally (and most important) to enact legislation will require simultaneous use of inside and outside strategies. Calling attention to an issue is insufficient; without adequate inside influence, members are unlikely to be able to advance their bills. On the other hand, media coverage may energize a legislative campaign that is languishing. In short, making news has become a central element in making laws; members seeking to shape public policy must have and use both legislative and media resources. In Cook's words: "Above all, the news media help House members set the legislative agenda, define the alternatives, influence public moods, and affect outcomes at a time when the political process is confused and unpredictable" (p. 169). Under these conditions, experts and leaders can take full advantage of their structural positions to advance the legislation they favor.

A subsidiary theme in Cook's analysis reflects differences between local and national newsmakers. Given the primacy of reelection interests, congressional press operations court the local media. Constituency news outlets, especially newspapers, are accessible, reach a wide audience, and have credibility; consequently, press secretaries focus on them initially. When attention shifts from reelection to policy making, however, the national media take on significance. Access here is more difficult to obtain: Washington reporters are selective, cover what they perceive as important matters, and are drawn to visible inside players. Fewer than half of the House membership get media coverage in any year, but members continue to seek national visibility as a means to move their programmatic causes ahead.

Cook has written a solid and significant book that offers a history and description of House press operations and a careful and persuasive analysis of new strategies. He moves beyond the broad generalizations of the conventional wisdom to provide a full picture of how the rise of the media has altered the policy making process in Congress. He is not altogether sanguine about the developments he assesses, noting that both the local media (with their excessively kind treatment of incumbents) and the national press (featuring its own definition of the newsworthy) have "unfavorable effects" (p. 175) on representative democracy. Citizens may not get a full or accurate portrait of legislative politics and congressional performance. Overall, Cook has contributed significantly to understanding and assessing the role of news and the politics of newsmaking in the House of Representatives.

LEROY N. RIESELBACH

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For the Common Good: Redirecting the Economy toward Community, the Environment, and a Sustainable Future. By Herman E. Daly and John B. Cobb, Jr. Boston: Beacon, 1989. 482p. $24.95.

Envisioning a Sustainable Society: Learning Our Way Out. By Lester W. Milbrath. Albany: State University of New York Press. 403p. $57.50 cloth, $18.95 paper.

The appearance of these two books is a cheering event, since both address the paramount problem of our time: designing a sustainable society. Furthermore, the authors—two social scientists and a philosopher-theologian—have clearly gone to great lengths to acquaint themselves with the physical, chemical, and biological dimensions of the human predicament. As a result, their view of the economic system is grounded in the real world rather than clouded by the simplified paradigm of the average economist.

For all the similarities of themes and references, these are very different books. The Daly-Cobb effort is philosophical and deep. It is by far the most scholarly critique of the dominant economic paradigm yet produced, building on Daly's own brilliant analyses and the pioneering work of Kenneth Boulding and Nicholas Georgescu-Roegen. It contains many intriguing new twists, such as comparing the way the concept of "externalities" is used to repair the shortcomings of market models to the way epicycles were used to repair the Ptolemaic astronomical model. As Daly and Cobb point out, when the capacity of Earth to support life must be classed as an externality, "it is time to restructure basic concepts and start with a different set of abstractions that can embrace what was previously external" (p. 37).

Daly and Cobb make a compelling case that the facts of the current environmental crisis simply fly in the face of the neoclassical economic paradigm. In particular, they expose the paucity of economic theory's regard for physical and biological constraints, although they fail fully to address the exact mechanisms of what economists call "market failure." Ultimately, no economist should be able to read this book without discomfort.

Unfortunately, it is difficult to imagine most economists reading the book at all. But we hope that enough other scholars will read it and put pressure on economists to revise their discipline and begin applying their considerable skills to helping solve human problems rather than exacerbating them. The book will be of great value to political scientists as well—in a minor way by helping them to avoid the pitfalls of physics-envy that have so injured economics and in a major way because of the large political component to the processes of decentralizing governments, revising educational systems, and taking other steps toward forming new kinds of human communities. The need to form such communities is a central theme of the book.

Both Daly and Cobb are Christian theists, and their beliefs are discussed in detail. Although I do not share those beliefs (and found their exposition somewhat tedious), I am in almost total agreement with their values. Indeed, if even a small minority of Christians, Moslems, and Jews adopted those values, the world would be a safer and more humane abode. I believe that *For the Common Good* is destined to be a classic; it is the collaborative product of two great thinkers. Unfortunately, it will be tough going for readers who are not well motivated; the material is dense and closely reasoned. It is a pity that there is no abbreviated and simplified version that could be recommended to every high school student.

Envisioning a Sustainable Society is basically intended as a college-level textbook and is more derivative. The literature that has accumulated on matters relating to sustainability since early efforts (e.g., Pirages and Ehrlich's *Ark II* [1974]) is impressive in quantity but very uneven in quality; and it is somewhat uncritically reported by Milbrath. The book is also infested with phrases such as "self-actualizing fulfilling life" (p. 82) and advice such as "Learn to think holistically, systemically, integratively." It also contains many scientific errors (such as assuming that the greenhouse effect is just beginning to be experienced, when in fact it has made Earth habitable). Fortunately, the errors do not detract from the book's general thrust; but social scientists should avoid involvement with fringe scientific concepts such as Sheldrake's "morphic resonance" and that notion that Earth is a superorganism—a living homeostatic system. In its weak form, the Gaia hypothesis is unexceptional; and its strong form is straightforward nonsense.

Despite these shortcomings, we would like to see college students everywhere exposed to this book. Milbrath is on the right track, and the exercise would be useful even if students only picked up his oft-repeated message that science is not value-free.

In sum, these two books deal with the most important problems the social sciences will deal with in the next few decades. The very existence of society as we know it is threatened by the scale of the human assault on Earth's life-support systems. No challenge to political scientists could be greater than helping to understand and alter the social system so as to ensure that it can operate successfully within severe physical and biological constraints. Read these books, and then act on them.

PAUL AND ANNE EHRLICH

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Campaigning in America: A History of Election Practices. By Robert J. Dinkin. Westport: Greenwood, 1989. 223p. $39.95.

Historian Robert J. Dinkin, author of *Voting in Provincial America* (1977), and *Voting in Revolutionary America* (1982), has given us a careful examination of the trends and evolution of electioneering from the colonial and revolutionary periods through the elections of 1988.

This is an ambitious book tracing the evolution of modern campaigning from its earliest times to the present; although thin on detail, it provides useful information for both academic and educated general audiences. Although the book is historical in approach, its utility for political scientists should not be overlooked. It provides a wealth of facts and anecdotes about specific candidates and campaigns, with a specific emphasis on the growth, development, and utilization of particular campaign techniques and practices.

In the first two chapters, which cover colonial and revolutionary campaigning and the electioneering forces in the early national period (1790–1820), the author informs the reader of several notable features of campaign style and practice in those early years. For example, in this interesting and informative book, we learn that the origins of such tried-and-true campaign techniques as the public speech and especially campaign debates can be traced to the colonial and revolutionary periods and that in the first truly contested presidential election (1796) the major issue was the personal traits and background of the two main contenders, Adams and Jefferson. It is both depressing and reassuring to discover that candidate image-centered campaigns have been a vital part of American politics since its earliest years. Also enlightening, in light of present-day emphasis on media-centered politics, is the fact that most of the struggle in this earliest contested race was carried out in the press, setting the stage for future contests. The author also traces to this race the creation of the first full-fledged campaign manager.

In the latter three chapters, Dinkin examines the "merchandised style" of campaigning and its evolution into the mass media age. For example, we learn that in the early merchandised era (1892–1920) the growth of complex issues such as free silver and increasingly independent-minded voters contributed to the growth of modern campaign advertising techniques because of a need to appeal to voters on a higher plane than mass rallies and free food and drink. Along with this came the adoption of modern advertising and increased salesmanship of candidates, the increased use of both objective and campaign produced printed matter and the decline of oratory.

With this new merchandised style came increased costs, leading to increased reliance on business to provide funding for campaigns. In this era, as in the latter half of the merchandised era (1920–52), as well as today, the Republican party led in the ability to produce more revenue and from more creative sources. This volume also traces the historical development of innovations such as radio, motion pictures, and later television and computers and how they came to be used in electioneering and came to be essential to campaigns.

This is really the first book to attempt to trace the evolution of campaign techniques throughout the entire history of the United States; as such, it succeeds in providing an excellent introduction to the topic. Because it is so ambitious, too many topics are treated superficially; but the work does a good job of cumulating a quite large body of historical fact and campaign anecdote in a quite readable volume. For greater depth of discussion or analysis of these trends in electioneering, the reader is advised to look elsewhere.

LISA LANGENBACH

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The Privatization Decision: Public Ends, Private Means. By John D. Donahue. New York: Basic Books, 1989. 264p. $22.95.

This book deals with an important question that is explored with admirable depth. Unfortunately, it is flawed by the restricted scope of its basic question. The title, *The Privatization Decision,* might be better rendered *The Decision To Contract Out* or *The Decision To Produce a Public Service.* Actually, the public has options beyond these two, which are essentially two ways to organize a supply response to government demand. In particular, there is true privatization—the requirement that private parties be the source of demand as well as supply. And of course, there are many forms of regulation that can range from general rules applicable to all market activity to specific constraints for individual products. The author can be deservedly praised for his sophisticated analytic probing of the private supplier versus public supplier choice. However, the book can be faulted for not embedding the arguments about public or private production within the broader context of how to design market and administrative elements in a sector with strong public interest.

The largest part of the book deals with four case studies of contracting: defense, local services, prisons, and job training. Donahue does a very careful evaluation of the literature to analyze the question of the wisdom of private supply being used to satisfy government demand in these cases. The case material follows several chapters where he states and applies the appropriate body of economic analysis to illuminate the contracting choice. The economic arguments are then used in the political economic context of the cases. Though there has been extensive writing on the desirability and flaws in public agencies' contracting out for production in these areas, these case studies have a freshness that derives from Donahue's combination of theory and evidence and his excellent writing style.

Donahue's arguments about the advisability of public production or private contractors are very much in the economist tradition of the analysis of markets, organizations, and agents. He does not offer any firm and hard rules but he analyzes the factors that make "privatization" more advisable. The most prominent factor in his analysis deals with the ability to identify and measure the output. The less clear the product, the more difficult it will be to write the specifications of the contract or to evaluate the performance of the contractor; therefore, the private contract becomes less advisable. Donahue says that the most significant factor contributing to possible success in contracting is the degree of competitiveness in potential suppliers: competition will force the suppliers to be efficient. The more risk associated with the product, the more the private supplier will try to add risk premiums and therefore lose his efficiency edge. The lesser the concern about the means of production relative to the value of the output, the more the government will be likely to trust a private contractor. (For example, the public will have very different views about the importance of procedures in prison care as against procedures in garbage pickup.) In reaching these conclusions, Donahue does a fairly thorough job (given space limitations) in discussing bureaucratic versus entrepreneurial behavior.

Donahue is careful to limit his study to the choice of contractor for public service, not the issue of reducing the public sector. Unfortunately, for the generality of his analysis, the factors that argue against private contracting also argue against the bureaucracy's being the supplier. It is possible that the service might be a candidate for denationalization rather than bureaucratization. For instance, consider the case of public education. Certainly, by Donahue's criterion it is difficult to define the output satisfactorily. However, this does not mean that we should leave it in the hands of the professionals, loosely constrained by elected commissioners. An alternative could be to encourage the private demand for, and private supply of, education. If purely private demand and supply resulted in a consequent loss in social values these could be offset by regulations and by voucher payments to the lower-income family. If the product cannot be properly judged by the collectivity, perhaps it should be left to the individuals to express their demand with private purchasers and to allow the suppliers to compete for the private dollars. The collectivity, if it thought that the aggregation of private demand through the market was not sufficient, has a variety of tools to correct the market, for instance, subsidies, taxes, and regulations.

This case of public education illustrates the general flaw of any study (no matter how excellent) whose scope is excessively limited. The general rule is that local optima are frequently inefficient when viewed from a more general case. To put it in more general language, a best solution where few alternatives are permitted may be a poor solution when more alternatives are offered. Public suppliers of schools may be preferred to private suppliers if the government budget pays for the costs. However, if private demand, supplemented by public subsidies to households, is allowed to control the market then a "true privatization" may be preferred. Subject to this significant limitation, Donahue does an excellent job of dealing with the problem of choice of public production versus public contracting with private suppliers.

JULIUS MARGOLIS

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Dilemmas of Presidential Leadership: From Washington through Lincoln. By Richard Ellis and Aaron Wildavsky. New Brunswick: Transaction, 1989, 245p. $39.95.

The President as Prisoner: A Structural Critique of the Carter and Reagan Years. By William F. Grover. Albany: State University of New York Press, 1989. 232p. $39.50 cloth, $12.95 paper.

Each of these two otherwise very different books seeks to place presidents within a context that shapes their possibilities for behavior. By its title, *The President As Prisoner* obviously implies that presidents have little maneuvering room to shape matters themselves.

Unlike much of the contemporary literature on the presidency that looks at the limits visited on presidents either by the nature of their strategic political situation or by the struggles within their administrations, both of these books look to very broad, if not grand, macroconditions. An important distinction between them, however, is that while Ellis and Wildavsky look to the interaction between the values of leaders and the cultural setting influencing the values of potential followers, Grover concludes that presidents are unable to lead because they must attend to the needs of corporate capitalism. Such needs presumably shape any president's behavioral priorities whatever his articulated agenda.

Grover's book is, thus, much less about the presidency (which the author largely sees as epiphenomenal) than about how, in the author's opinion, choices are structured in U.S. society by the omnipresent needs of corporate capitalism. The notion of presidential leadership, therefore, is largely illusory because, ultimately, presidents are prisoners of those needs.

Although both books suffer from flaws that are endemic to the enterprise of trying to bring large thoughts and grand themes to the problem of evaluating leadership, the Grover volume fails to put forth either a logically consistent argument or an empirically meaningful assessment of that argument (were it logically consistent). The book eventually reads much like a diatribe against the reputed dominance of corporate capitalism and its needs (never defined) and the dominant class that it has bequeathed.

A case can be made about the linkage between economic imperatives and the limits imposed on political leadership, but that case has not been made here. Indeed, the book takes an important question (In what ways do the requirements of capital investment constrain the menu of public choice?) and even an important perspective on that question (the neo-Marxian critique of the state as a shelter for capital) and then trivializes them both through a garbled and largely ex cathedra commentary.

Grover's approach is to critique the presidency literature from the perspective of a broader societal structural approach—a reasonable enough venture. He then proceeds to lay on a thick overcoat of neo-Marxist literature on the state. The author then tries to demonstrate the value of this perspective through the use of two cases: (1) conflict over regulation and economic growth imperatives in regard to the Occupational Safety and Health Administration (OSHA) and (2) the strategic debate regarding the MX missile. Keep in mind that in both cases the results are presumed to be a consequence of the needs of corporate capitalism.

The OSHA debate surely does demonstrate that there is a tension between the desires of regulators of business to regulate and the desires of business (often small firms or agricultural producers) to escape the regulatory burden as much as possible. OSHA got into political trouble through doctrinaire leadership, and the not uncommon sin of nurturing its closest clienteles—in this case, the AFL-CIO and various occupational health and safety specialists. OSHA went for the big bundle during the Carter administration and failed in part because a deregulatory syndrome already had set in and because the agency was well known for its regulatory heavy hand—a hand that the Reagan administration then put in cold storage. In no way does the author suggest a test of his hypothesis that corporate capital single-handedly weakened OSHA. Rather, he asserts it; and that (supposedly) is that.

The MX case is even further from any reasonable inference or test. In essence, the author talks about the MX in the context of the Cold War, which he sees as largely feeding the needs of corporate capitalism. Historically (and especially during the Reagan years), the struggle between corporate capitalists who wanted to do business with the "enemy" and the government that wished to deter them was legendary. It is, indeed, quite stunning to see how rapidly other corporate capitalists around the world have been interested in investing in the former socialist bloc economies. They must be operating somehow on imperatives other than those of their U.S. counterparts.

Finally, Grover muses over whether it is possible that the prison in which presidents are reputedly incarcerated can be destroyed. He rightly suggests that sometimes political science theorizing about the presidency is too bounded in concept. But he, in turn, has failed to give us the broader one he has in mind. At this juncture, Grover waxes poetic: "To understand one (incarceration) is to have a more realistic shot at the other (liberation). Prisons need not hold us forever. Again, structures can change." Alas, Grover neither clearly defines the structures nor how they might change. The book, in this regard, contains more rhetorical flourishes than theoretical analysis. And whatever one's opinion of the book, for $39.50, one could also expect a better job of editing out typographical errors.

Dilemmas of Presidential Leadership is a far more interesting book. Its argument is that the presidency ought to be seen as a continuous office in which similar dilemmas of leadership are faced by its incumbents even as the office and its responsibilities change in more superficial ways. For Ellis and Wildavsky, distinguishing between a modern and premodern presidency makes little sense. Therefore, in the essentials, it is entirely possible and certainly desirable for us to compare presidencies over time. The key is to find the right basis on which to do this.

That basis, according to Ellis and Wildavsky, is to be found in the relationship between *leaders* and *followers.* Ellis and Wildavsky look at the congruence between leadership values and those prevalent in the society. In this regard, they posit three sets of value systems or cultures: egalitarian, hierarchical, and individualistic. U.S. exceptionalism is ascribed to the peculiar confluence of individualistic and egalitarian strains in the culture. The crucial leadership dilemma for any president is the matching of his values with those that are prevalent in the followership. The authors then attempt to examine clusters of presidents in chronological sequence from this values perspective. Their method is essentially that of tracking the secondary historical literature and drawing inferences from that.

The ideas are exceedingly interesting; their execution may be more debatable. The authors are correct in arguing that we have neglected the earlier presidents because we have drowned them in historical hyperfactualism. We have been without guidance in assessing them because of the absence of a common frame of reference. But reducing presidential performance to conflicts or congruencies of values may be a bit strained. The values focus neglects the mechanisms of politics and the means of aggregating or disaggregating preference. Not so long ago, when it was not very clear what Jimmy Carter's values were, blame for his problems was mostly cast in distinctly political rather than value terms. The problem of aggregating preferences certainly had to do with failures of leadership strategy; but they also had to do with the structure of interests able to deter ambitious programs that often called for immediate sacrifices. His problems also had to do with his political party and the difficulties each had in deciphering one another. Moreover, at any given time it is not abundantly clear what public values are, since they are often fluid. Excesses in one direction tend to beget swings toward another. The authors wind up, in my view, closer to the mark by suggesting that presidential fate is often closely linked to the products of a president's predecessors.

Ellis and Wildavsky put forth a set of ideas that help us think about the problem of leadership. The ideas lack both tight conceptualization and a clear standard of operationalization. Nor are they evaluated against any plausible set of alternatives. They are, however, stimulating and provocative; and they force thinking outside of the normal discourse of the analysis of presidential leadership. Above all, they engage the question of what leadership actually is. That's worth two cheers, anyway.

BERT A. ROCKMAN

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The Republic of Choice: Law, Authority, and Culture. By Lawrence M. Friedman. Harvard University Press, 1990. 248p. $27.50.

In *The Republic of Choice: Law, Authority, and Culture,* Lawrence Friedman has made an intriguing and important contribution to our understanding of law in society. How do we explain the so-called "litigation explosion" in the United States? Why do we have so many lawyers relative to our population, our history, and other societies? What is there about how legal questions in the United States are formulated and discussed that makes them unique to this society at this time? Are other Western cultures following our lead and if so, how? More generally, what is there about our culture—indeed, about Western culture at the end of the twentieth century—that helps explain these peculiar developments?

In seeking answers to these questions, Friedman ranges widely and provocatively into political philosophy, popular culture, social theory, economics, history, and anthropology. His inquiry knows no disciplinary boundaries. It is the work of a restless, wide-ranging, and even somewhat impatient mind. The undertaking is ambitious, to say the least. The cultural foundations of law and the puzzling ways in which law both manifests and structures social relationships have occupied scholars through the centuries. In recent years, inquirers have ranged from Harvard's Roberto Unger to the U.S. Court of Appeals' Richard Posner. That Friedman manages to bring new light to this sometimes obscure discussion in just 206 pages of text is all the more remarkable.

Put simply, Friedman posits a *republic of choice,* best exemplified by modern U.S. culture, in which freedom is understood and experienced as a series of personal choices—about work, political affiliation, social affiliation, life-style, and family. To be fully human in this culture is to assert control over one's life and thus to make such choices unimpeded by the weight of obligation, tradition, or social norms.

Law, within this republic of choice, is not imposed on the population, as it was in more primitive societies. Nor is it understood, for the most part, as a series of commands, duties, and constraints. It is, rather, another instrument of personal choice. Laws and legal institutions are themselves chosen; if they do not fulfill their instrumental task of enhancing and guaranteeing personal choice, they can, presumably, be changed.

Relationships within the republic of choice are based primarily on contract. Terms are determined in advance by freely consenting individuals and can be altered. This paradigm is, of course, the legacy of nineteenth-century liberalism. But there is an important difference: in the nineteenth century, contracts were embedded within a tacit system of norms and institutions that enforced social discipline and self-control. "In fact," writes Friedman, "expansion of freedom—expansion of the zone of choice, *depended* on self-control" (p. 35; emphasis original). Who would contract with someone who could not be counted on to fulfill the agreed-on obligations? The world was a cruel and dangerous place, especially for those lacking in self-discipline.

In the modern United States, however, the overriding goal is not self-control but self-actualization. We socialize our children not to develop their character but to develop their personality. The quest for social order, in other words, has been replaced by a societal thirst for expressive individualism. Norms that had once been tacitly enforced by the community are now vague or unenforceable.

The transformation has many causes—economic, demographic, technological. For one, most of us live in a far wealthier society than before; the threat of social ostracism is thus a less potent inhibition on self-seeking behavior. For another, we are apt to be surrounded by relative strangers: Even to a greater extent than our forefathers and mothers, modern U.S. citizens are highly mobile both geographically and socially. Technology further expands our reach beyond familiar webs of friends and family. In this fluid and dynamic world, reputations do not last long or count for much. Thus, social contraints are even less effective.

Law must fill the ever-widening breech. Contracts are necessarily more complex, property rights more specifically defined, liability broader. In the republic of choice, social relationships become defined by the self-actualizing possibilities of oneself and others. Each person needs and demands so much room that the chances of one person's bumping up against another are greatly enhanced. Law is repeatedly invoked and clarified as individuals test the outer boundaries of their personal choices. Employees and employers, landlords and tenants, spouses, children and parents, neighbors, doctors and patients, consumers and sellers: all are in more or less continuous legal combat.

Law also becomes a vehicle for redress in the face of calamity, bad luck, or perceived unfairness. In the republic of choice, no social penalty attaches to testing such claims; one's jobs, apartment, or other station is guaranteed against recrimination. Access to "justice" is a cardinal principle, protected by the state.

In fact, argues Friedman, the entire notion of *entitlement* has its roots in the culture of personal choice. In the eighteenth- and nineteenth-century formulations, rights and entitlements were accompanied by explicit duties and obligations. But the social contract of the republic of choice is asymmetric: one is free to claim entitlements from the society, but society is not permitted to impose burdens in return. This is because there is no society as such, only a vast collection of individual claimants. The only clear limits on such claims are understood to be the claims of others.

Perhaps I am extending Friedman's argument further than he wishes it to be extended, suggesting that he is far more critical of these cultural changes than he actually claims. His argument is subtle and clever, and he avoids sanctimony. Most of the book is description and explanation, rather than judgment. And when he embarks on an assessment, in the last chapter, he is careful to point out the attractions of the republic of choice: litigation and liability may be a price worth paying for the kind of personal freedom enjoyed by twentieth-century U.S. citizens. To the extent that social justice is enhanced, the price may be small.

If the book has a failing, it is its breeziness. Friedman's argument is so brisk and travels over so much terrain in so short a time that the reader is left breathless. More specific illustrations, longer discussions of individual cases and streams of legal analysis, and more thorough historic exigesis might have aided the reader who was less than willing to take Friedman's word for it.

There is also the question of the extent of the republic of choice: Is he describing the modern United States or modernity itself? Friedman suggests that the phenomena he is describing are gradually extending to most of the West, but he fails to explain exactly why or how. Further, is he describing the modern United States or just the modern U.S. *male?* He devotes several paragraphs to the question whether feminism poses a fundamental challenge to his thesis (and the work of many feminist scholars would suggest that it does), but the reader is left with the same slight sense of lightheadedness; surely this issue deserves more than a few paragraphs?

These problems aside, Lawrence Friedman has made a signal contribution to the study of law and society. His bold interdisciplinary approach is a model of interpretive scholarship at its best and most buoyant.

ROBERT B. REICH

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Ironies in Organizational Development. By Robert T. Golembiewski. New Brunswick, NJ: Transaction, 1990. 303p. $34.95.

"The propositions of small group research—when we have them—will be found deducible from a general psychology of behavior—when we have it," asserted George Homans some 20 years ago. Alas, we're still waiting for both. Few students of small group research have slashed at these two Gordian knots more mightily than Robert Golembiewski, whose zesty and prolific writings—over three hundred articles and an endless series of books—make him the Joyce Carol Oates of scholarship in organizational development.

The irony of ironies in organizational development, Golembiewski claims, is that "it succeeded too well and too fast" (p. 282) *before* the propositions of small group research (whatever they are) were deduced from a general psychology of behavior (whatever that is). Today, the core values of organizational development—promoting freedom, responsibility, trust, and "regenerative interaction to replace the degenerative variety" (p. 233)—are almost universally lauded by management and labor alike. Canvassing published and unpublished sources, Golembiewski marshals persuasive evidence that the success rate of organizational development interventions (even in the public sector, and in Third World settings) is astonishingly high. Yet Golembiewski is troubled. He finds little more than "conceptual myopia" (p. 111) in the babel of organizational development literature. Intervenors may be brilliant in improvising strategies for almost any setting; but neither they nor organizational development theorists can explain why—or under what specific conditions—change occurs.

Where is the *science* of organizational development? Golembiewski asks plaintively. When can we, at long last, begin to predict the very best modes of intervention for specific contexts, in order, ultimately, to "substantially enrich praxis" (p. ix)? How ironic that the most successful organizational development practitioners "are seen as running ahead of our conceptualizing and theorizing about what to do and why." This "short-run success," Golembiewski warns, "can be long-run poison" (pp. 3–4). We had better begin to mount "a kind of Manhattan Project for the organizational arts and sciences" (p. 287), a mammoth project to finally produce a valid, reliable, and manageable number of configurations or covariates for the theory of organizational development. Golembiewski fears that if this project does not begin soon and if practice continues to outpace theory, theory will "collapse on itself" (p. 4) and, presumably, fall down on the heads of organizational development theorists like himself.

While sympathetic to Golembiewski's plea for better and more theoretically informed organizational development, I wonder if he hasn't overlooked an even larger irony in his quest to relieve the theorist's "metaphysical pathos" (p. 88). Positivist science—which produced the ambiguous atomic products of the Manhattan Project—worships only at the feet of the bitch goddess quantification and her handmaidens Prediction and Explanation. The big irony missed by Golembiewski is that there's simply no room for basic organizational development values (freedom, responsibility, choice) in the "normal science" agenda he advocates for closing the gap between theory and practice. At best, positivist research is scientific; at worst, scientistic. In either case, organizational development values can only be seen by positivists as a return to metaphysical pathos, not as an escape from conceptual myopia.

Is it social science heresy to propose that the success rate of organizational development interventions may have just a fleeting, tangential relationship to the intervention mode or even to the characteristics of the intervenor? Then let's become heretics. If we truly believe in human freedom, aren't we forced to conclude that intervenors cannot "engineer" freedom or "induce" change by applying and refining organizational development "technology" (a word that Golembiewski and others in the field unfortunately use with no sense of irony whatsoever)? Like psychotherapists, organizational development intervenors are servants—often just bystanders—of a process in which, stimulated by the Hawthorne Effect, the creative energies of living people unfold in mysterious, usually unpredictable, ways. Whether organizational development researchers use the experimental, quasi-experimental, or correlational methods of "normal science" or the deeper interpretations of psychoanalysis, the reasons they discover for "why" individuals or organizations change should be seen *by definition* as nothing but codicils to change, not the causal antecedents of change.

Alfred Korzybski once observed that the illusion of clarity increases with the level of abstraction. Kurt Lewin, the father of action research and laboratory (*T*-group) training, may partially be held responsible for the organizational development theorist's metaphysical pathos by arguing for the *group* as the preferred unit of analysis, instead of the intersubjective social self, for example, of George Herbert Mead. With the group as the unit of analysis, we can only marvel at Golembiewski's dazzling, but ultimately unsatisfying, statistical pyrotechnics, which have the unintended effect of hiding human freedom inside group means, Ahmavaara's elegant procedure for comparing factorial structures, and Golembiewski's well-known alpha, beta, and gamma models of change.

To forestall theory from collapsing on itself, let us once again take seriously our own unshakable commitment to freedom as the core organizational development value. Perhaps we should differentiate two variants of freedom, two forms of human agency: (1) the actions (and reasons) of *intervenors*, presumably free but bounded by the limits of self-awareness and scope of the organizational development design and (2) the actions (and reactions) of the *subjects* of the intervention also free and bounded by the limits of self-awareness only. For too long, some intervenors (and some theorists) have tacitly accepted just the first kind of freedom, namely, their own. Grizzled organizational development veterans and post-Freudian therapists know that they succeed only when they accept both kinds of freedom: their own *and* their subjects. If determinism rules, why bother with organizational development? Why bother with individual development? Why bother with therapy?

As this book shows, Golembiewski has few peers in the imaginative exploration of research protocols to enhance organizational development "technology." For those who see technology as the correlative of scientific knowledge, there is no better book available. But surely the freedom lover hiding in Golembiewski-the-theorist remembers that *techne* originally meant "art," not technical knowledge. And art, if it means anything, means freedom.

ROBERT KRAMER

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On Strike at Hormel: The Struggle for a Democratic Labor Movement. By Hardy S. Green. Philadelphia: Temple University Press, 1990. 368p. $29.95.

The 1985-86 Austin, Minnesota strike against Hormel, widely called the "strike of the decade" was, indeed, one of the bitterest labor disputes of the 1980s. The Hormel strike became a symbol of part of the labor movement's rejection of concessions. This is a firsthand account of the struggle at United Food and Commercial Workers (UFCW) Local P-9.

Hardy Green, who now writes for *Business Week*, participated in the P-9 strike as a staff member for Ray Rogers' Corporate Campaigns, hired by the local to devise and implement overall strategy. The advantage of this position is that Green was privy to behind-the-scenes tactical discussions. He brings insights to the struggle that would be difficult, if not impossible, for an outsider to provide. Those who seek a detached, dispassionate account of the Hormel strike, of course, will be disappointed: Green makes no secret of his partisanship. Yet he is able to come to terms with the motivations of different actors, even those he disagrees with—particularly on the union side. He fairly presents the positions of the UFCW International leadership and Lane Kirkland, president of the AFL-CIO. He shows that Rogers' "corporate campaign" strategy has met with mixed success.

The book provides a rich, moment-by-moment description of the events before, during, and after the strike. Green relies on his own recollections; interviews with participants; and sources such as media accounts, court testimony, and police files. The result is a riveting description of the Hormel strike. Green emphasizes that workers entered the battle with a fairly conservative aim—to protect their middle-class living standard—only to be driven into increasingly militant actions by corporate intransigence. As a result, their consciousness was transformed. By the end of the strike, the predominantly white, male workers of Midwestern small towns were dedicating a mural painted on their union hall to Nelson Mandela. Workers conceived of their own struggle as one for dignity, not just wages, and came to understand its connection with other struggles for social justice.

Green emphasizes the changing dynamics of the Hormel strike. His account weaves together the intentions and strategic choices of local leaders, the UFCW International, the company, and public officials. His description is so painstakingly careful that it is possible to judge from his narrative the tactical turning points in the struggle. For instance, in early 1986, with the strike several months old, P-9 leaders chose to picket other Hormel plants, even though the company was operating the Austin plant with unionists who crossed the picket lines and "replacement workers." Green states that "among the Austin ranks, there had always been strong sentiment that P-9 could not expect other Hormel workers to honor their pickets if the local could not keep its own Austin plant closed down" (p. 195). Indeed, the extended pickets at Hormel plants in Nebraska, Iowa, and Wisconsin were only marginally successful. Might a clearer emphasis on obstructing production in Austin have been more successful?

The P-9 strike became the focus for debate throughout the U.S. labor movement. The official labor leadership argued that militant grass-roots struggle against concessions was unwise and doomed to failure. P-9 and its supporters, including thousands of local leaders and rank-and-file activists, contended that the right tactics and union solidarity could turn the tide against concessions even in the midst of a decade of defeats for organized labor. Ultimately, this division helped to strangle P-9, for the struggle was not definitively over until the UFCW leadership put the local into trusteeship. By arguing the P-9 case persuasively, Green makes a significant contribution to the wider argument in the U.S. labor movement over the strategic response to the employers' offensive.

Despite the subtitle, Green does not attempt to set his discussion of labor tactics in the wider context of literature on democratic organizations. He makes only passing reference to Michels and none to recent literature on union behavior informed by democratic theory. Still, this book is an important contribution to the descriptive literature on labor conflict. As social scientists are increasingly concerned with the microfoundations of group behavior, such descriptions are highly valuable.

GLENN PERUSEK

Albion College

Speech, Crime, and the Uses of Language. By Kent Greenawalt. New York: Oxford University Press, 1989. 349p. $45.00.

Ascertaining the dividing line between legally protected free speech and other speechlike acts of communication has been a perennial problem for the courts and First Amendment scholars. Greenawalt's goal is to construct a theory that permits legislatures to distinguish communication that is supported by the principle of free speech from other acts of communication (such as extortion, threats, and fraud) that are really crimes. At the same time the author also wishes to defend a "libertarian" theory of free speech that recognizes a maximum amount of unconstrained communication within a free society.

The book divides into three sections with the first providing both a defense of free speech and a theory of what *speech* is. In chapter 2, Greenawalt reviews general and specific defenses of why free speech is important, contending that classic J. S. Mill defenses of free speech as essential to the production of truth do not make sense if we reject independent notions of truth. Defense of free speech must rest elsewhere, then; and the author concludes that communication, both private and public, is important to democracy, public discourse, and private catharsis.

Chapter 3 is the theoretical core of the book and the author claims to rely on J. L. Austin's theory of speech-acts as a means to distinguish speech from other forms of communication. According to Greenawalt, he narrows Austin's conception of "performative utterances" to *situation-altering* actions (p. 58) and argues that the principle of free speech protects utterances that are primarily assertions of fact and value while *nonspeech* entails acts of communication that are primarily situation-altering or aimed at changing the world. While some utterances may both express fact or value and be situation-altering, we need to ascertain which is primary to determine whether it is really speech.

The second section is an extremely interesting discussion of acts of communication that are not really speech but instead are crimes. Greenawalt argues that legislatures can distinguish the crimes of fraud, threats, blackmail, and so on by applying the situation-altering distinction. Free speech scholars and general readers will find his discussion of various kinds of speechlike crimes and acts to be novel, engaging, and thought-provoking.

The third part of the book is a legal analysis of the First Amendment that again relies on the situation-altering distinction. Here the author discusses free speech jurisprudence and the failures of rival and past Supreme Court interpretive techniques (such as Bork's or Meiklejohn's) to define the scope of our free speech protections under the First Amendment. The main point of this last section is to connect his theory to the Court's test of free speech in *Brandenburg* v. *Ohio* and to show the superiority of his distinctions to other tests including "clear and present danger" tests, as able to defend a very broad theory of protected speech.

While there is much to like in this book and while it does address some critical issues in the law, ultimately, its theoretical core is weak; and it is not clear that Greenawalt's situation-altering test for speech can either hold up in reality or theory. First, I am not sure the author really uses Austin's performative utterances notion correctly or usefully. Second, while he wishes to say that real (protected) speech is, or ought to be, non-situation-altering, he ignores how all speech, however remote, ultimately aims at changing the world, even though it is not immediately performative.

Additionally, Greenawalt's theory would be difficult to apply to symbolic, or speech-plus, cases such as flag burning and sit-ins (e.g., *Brown* v. *Louisiana).* Even Greenawalt admits that there are problems with his theory and that it is difficult to accommodate it to all of the distinctions he wishes to make in the last part of his book. In the end, there is a very ad hoc character to his discussion of free speech. Other problems with the book include the repetitious character of parts 2 and 3, the failure to engage recent critics of First Amendment jurisprudence such as Bork more thoughtfully, and the exclusion of any extended treatment of many important speech issues such as obscenity and pornography.

Overall, the book is to be praised as a serious attempt to defend and explain what free speech is in a liberal society; but Greenawalt's failure to provide a theoretical distinction is representative of discussions of the changing social conceptions of what free speech ought to be in our society and of the inability of the Court and scholars to capture the boundaries of that dynamic change.

DAVID SCHULTZ

Gustavus Adolphus College

Tribune of the People: The Minnesota Legislature and Its Leadership. By Royce Hanson. Minneapolis: University of Minnesota Press, 1989. 278p. $29.50.

This book is one of the products of a three-year study of the Minnesota legislature, carried out by the University of Minnesota Hubert H. Humphrey Institute of Public Affairs with the support of the legislature. Royce Hanson, who directed the study, enlisted the help of a number of Minnesota political scientists on various aspects of it. The origins and purpose of the study have shaped its content: the emphasis is on legislative structure and process. Relatively little attention is paid to other topics of interest to political scientists, such as legislative elections and campaigns, the representative process, or the roles played by legislators in their districts. It is based in large part on surveys of members and interviews with legislative leaders and former leaders. It also reflects the comments of members on reports made to the legislature during the study. This helps explain the realistic and pragmatic tone of the book.

Some sections of the book describe problems facing the Minnesota legislature, defined as "crises of competence, legitimacy, leadership, and accountability"; and the final chapter proposes a number of solutions. For example, Hanson argues that the best way to achieve accountability is through adoption of a unicameral system (a recommendation that has not been embraced eagerly by the legislature). These parts of the book are probably of only marginal interest to non-Minnesota political scientists.

The value of this book for political scientists lies in its detailed, realistic description of the evolution and current functioning of the Minnesota legislature. Students of comparative legislatures are handicapped by the shortage of such studies in individual states.

There is an excellent analysis of the characteristics and strategies of a number of the recent legislative leaders. Closely related to this is an examination of the changing role of party caucuses and the growing importance of partisanship in decision making.

Conference committees play a major role in Minnesota, and this book provides one of the most thorough and insightful analyses to date on how state legislative conference committees work. Equally valuable is the analysis of the budgetary process in Minnesota, in which Hanson argues that changing economic realities and political conflict have undermined the effectiveness of the existing process, much as they have done at the congressional level. The author suggests that major changes are necessary in both the conference committee and budgetary processes.

What seems to be lacking in this book is a comparative, cross-state perspective. Hanson does not seem to have made much use of the literature in this field—or at least does not cite much of it. He makes little effort to show how recent trends in the Minnesota legislature and the problems it faces compare with other states. (For example, compared to most states, Minnesota appears to have stronger party caucuses and conference committees and has not followed the trend toward less turnover in membership; but its difficulties in making the budgetary process work are comparable to those in many states.)

One of the major questions of current concern to both scholars and practitioners is the changing role of parties and the growth of partisan conflict, trends that seem to affect many states. Hanson points out that in Minnesota there is some concern about growing partisanship, fueled by frequent changes in party control of the House; and he argues for greater protection of minority party rights. But his analysis and recommendations would be more valuable if he took into account some of the trends and controversy over partisanship occurring in other states.

Despite its almost exclusive focus on one state, this book will be valuable to students of comparative state legislatures because it describes in realistic detail trends and problems in Minnesota that are similar in most respects to those we find elsewhere.

MALCOLM E. JEWELL

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Making Regulatory Policy. Edited by Keith Hawkins and John M. Thomas. Pittsburgh: University of Pittsburgh Press, 1989. 282p. $34.95.

This assessment of regulatory policy making is grounded in two administrative perspectives—*bounded rationality* and *social construction*—and focuses on the key issue of agency discretion. Three themes are supposed to guide the analysis: the information so critical to rule making is subjected to influence within the regulatory organization; the methods to implement policy goals have a relationship to external constituencies and interest groups; and the enforcement process necessarily involves the issue of discretion.

In part 1, one chapter is a too-abstract characterization of the kinds of personal influences that conceivably may have shaped governmental research on the problem of victimization in Canada, while another discusses the nuclear power accident of Three Mile Island to illustrate the limits of scientific knowledge and rationality (compared to the importance of "trust") in regulatory enforcement.

Part 2, the real contribution of this volume, consists of three detailed case studies to show how rule making, methodology, and procedures are shaped both by policy goals and by outside groups that desire to influence the policy process. One study gives in rich detail a postmortem of the shift by the Federal Trade Commission from "capture" to zealotry and return to passivity in the face of demands for consumer protection; a second briefly accounts for why the Environmental Protection Agency (EPA) backed away from more aggressive regulation of chlorofluorocarbons; and the third is a well-crafted discussion of how the EPA abandoned its traditional command-and-control approach and moved toward developing a policy on emissions (permits) trading as an alternative mode of air pollution regulation.

In part 3, one chapter offers a more theoretical argument that linkages between agency rule making and discretion are influenced by their compliance effects as well as the type of cases encountered (such as recurring or nonrecurring); and the concluding essay summarizes the implications that bounded rationality and social construction have on designing regulatory policy.

The analysis is heavily legalistic, as all but two of the contributions are written by law faculty; there is too much logic chopping based on abstract formulations; and much of the theoretical discussion focuses on whether more "comprehensive rationality" can be achieved instead of policy "incrementalism." All this flows implicitly from the assumption of a neutrally competent bureaucracy, with the result that the manifest concerns about rule making and procedures are debated in a context that fails to give serious attention to the critical importance of political variables.

The roles of interest groups and external constituencies are explicitly recognized but are never specified as causal factors; and, while the editors further acknowledge that regulatory policy is highly conflict-laden and involves trade-offs within and outside the agencies, they give no consideration to the contrary view (especially apt, given that the Federal Trade Commission [FTC] and EPA cases showed the decisive impact of the Reagan administration) of a politicized government resulting from presidential appointments and executive oversight. However, the case studies (notably the FTC chronicle) reveal that the key turning points in regulatory policy were decisively influenced by profound changes in the political environment.

The reader is constantly told that regulatory policy making is highly complex and interrelated with environmental factors, but the essential argument of this volume is that as singular approaches, neither bounded rationality nor social construction is enough. Both interactive processes must be taken together to achieve a more realistic—even if more complicated—view of the regulatory process.

The serious deficiency of this legalistic approach to case studies is that the book promises no theoretical breakthroughs. While Ted Lowi originally limited his policy typology to the enactment stage, the administrative processes underlying the kind of "protective" regulations included here had been sketched out in later analyses by Randall Ripley and Grace Franklin. But that literature is barely even mentioned in these chapters, though it offers a critical point of departure for what this book proposed to do, namely, take "a preliminary step toward the development of more inclusive, general framework for understanding the phenomenon of *how* regulators actually regulate" (p. viii).

Simply put, a theoretical framework should have been adopted conceptualizing the policymaking process either as a "balance of power" among contending political actors (within and external to the agency) or as a "field of forces" acting as constraints on bureaucratic decusion makers. After the variables are identified and their interrelationships specified, numerous hypotheses could have been offered as a research agenda for future validation.

RAYMOND TATALOVICH

Loyola University of Chicago

The CIA and American Democracy. By Rhodri Jeffreys-Jones. New Haven, CT: Yale University Press, 1989. 358p. $25.00.

Beginning in the 1960s, there has been a growth industry for books on U.S. intelligence, especially the CIA and its covert operations. Many of the earlier books were written by insiders who were either promotive and apologetic about the CIA or extremely critical. However, the last decade has produced a spate of books taking a more analytical and scholarly orientation. *The CIA and American Democracy* is symbolic of this recent development. Rhodri Jeffreys-Jones, a British historian who has written extensively on intelligence, has produced one of the most comprehensive and valuable treatments of the history of the CIA and its place within U.S. democracy.

This is not just another book describing the CIA's covert operations. Going beyond description, Jeffreys-Jones provides a very balanced and analytical account of the evolution of the CIA, its relationship to U.S. politics, and its consequences for U.S. foreign policy. A number of important themes or topics run through the book, including the crucial role of politics on the CIA, the importance of the CIA–White House relationship, the importance of the CIA's other intelligence functions in addition to covert operations, and the difficulties involved in conducting clandestine intelligence in a democratic polity.

The most important point that Jeffreys-Jones emphasizes throughout the book is the crucial role that domestic politics plays in determining what he calls the CIA's "standing"—that is, legitimacy and credibility in the minds of U.S. citizens and policy makers. The crucial determinant of the CIA's standing is politics; and as U.S. politics has evolved, so has the CIA—from its rise during the late 1940s, to its golden age during the 1950s, decline during the 1960s, fall and renewal during the 1970s, and resurgence during the 1980s.

The very creation of the CIA by the National Security Act of 1947 was a function of political compromise. While some believed that a centralized intelligence agency was necessary, some feared the creation of a superspy agency reminiscent of the German Gestapo, and some, especially within the military and the FBI, wanted to protect the vested interests of other intelligence agencies that had been in existence for some time. Since its creation, it has been critiqued by the political Left for being too unaccountable, too secretive, and too driven to excess in its covert operations and by the political Right as being too dovish in its intelligence estimates and operations toward the Soviet Union and communism. Politicians, especially in Congress, have been all-too-prone to support or critique the CIA in response to the dominant political winds rather than opting for dispassionate analysis in the name of balancing the requirements of national security and democracy.

Jeffreys-Jones also emphasizes the importance of the CIA–White House relationship. The CIA has acted as both a source of, and a constraint on, presidential power in foreign policy. During the Cold War years, presidents came to rely on the CIA to provide information for presidential decisions and on its covert operations as a crucial tool in support of the policy of containment. However, when intelligence failures occur—no matter how inevitable—or when unsavory covert operations come to light, the CIA becomes an embarrassment for the U.S. government and the president. During these times, presidents have tended to treat the CIA as a scapegoat to deflect political attention away from them and their policies.

Unlike most books on the CIA, Jeffreys-Jones discusses the important role of intelligence (data collection and analysis), science and technology, and espionage in addition to covert political paramilitary operations. He makes it clear that the CIA is a large and complex bureaucratic organization with multiple intelligence functions that have become increasingly professionalized over time. He points out the difficulty that the director of Central Intelligence has had in coordinating national intelligence estimates with the larger intelligence community. He also discusses how presidents, especially Johnson, Nixon, and Reagan, have pushed certain foreign policy agendas that have made it very difficult for the intelligence analyst to provide honest information and conclusions that vary from the beliefs entertained by the President and his closest advisers.

Finally, Jeffreys-Jones discusses the importance of balancing the need for an intelligence capability and democratic accountability. He argues that reform can be accomplished through the development of legal guidelines through legislation signed by the president. However, for a variety of reasons, this is easier said than done. First, he underplays the role that politics has had in preventing the kind of intelligence charter that he believes will provide the CIA with greater standing and continuity in its intelligence activities. Second, the prevalence of a security and secrecy ethos throughout the government has made it difficult to discuss rationally—let alone agree to—a set of standards that balances the demands of national security with the demands of democracy. Third, he ignores the necessity of achieving internal CIA reform, especially in the operations division and among the CIA's leadership, where the "cowboy" subculture has dominated throughout much of its history. The CIA's incompetence, its many failures, and its national security excesses involving cloak-and-dagger operations have less to do with the CIA's standing than with the structures and subcultures that prevail within the agency. Finally, he ignores the importance of modifying U.S. foreign policy and the strategy of global containment that has been responsible for producing a large national security bureaucracy for fighting a Cold War over the last four decades. Thus, the resurgence of an unreformed CIA and covert wars under President Reagan in a post-Cold War political environment was a sure recipe for what became another political scandal—the Iran-Contra affair.

Overall, Jeffreys-Jones has produced an impressive book that is extremely informative and well written and addresses critical political issues. The bibliography is up to date, and its breadth is most impressive. Most importantly, *The CIA and American Democracy* is successful in getting the reader to understand and reflect on the precarious interaction between democracy, politics, and intelligence.

JEREL A. ROSATI

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America's Secret Power: The CIA in a Democratic Society. By Loch K. Johnson. New York: Oxford University Press, 1989. 344p. $24.95.

Are secret intelligence organizations compatible with U.S. democratic values and processes? Johnson's latest—and best—book answers this question by examining the "proper boundaries for intelligence activities, and the chances that overseers can maintain them" (p. 11). In analyzing the nature of such boundaries and describing how they have been breached, Johnson breaks new theoretical ground for students of both national security and the ethics of foreign policy and adds important data about many contemporary intelligence activities to the public record. The chapters on covert action operations and executive and legislative oversight are particularly good and well documented. Administrators and professors of political science at major research universities will especially profit from the chapter on CIA relations with the U.S. academic community. Johnson gives the reader interested in this topic a much-needed update to Robin Winks's superb *Cloak and Gown: Scholars in the Secret War, 1939–1961* (1987) and some excellent advice about how to manage the relationship. And the several chapters devoted to diagnosing the pathologies in the intelligence system and proposing remedies should be required reading for congressional staffers whose members are involved in overseeing intelligence.

But the book is neither another I-told-you-so exposé by a former congressional staffer (which Johnson was in the 1970s) nor an effort to use a prior position involving access to intelligence methods to reveal sensational details of how the United States spies on other countries.

Johnson begins, instead, by reminding the reader that U.S. citizens have never really been comfortable about funding a permanent peacetime secret intelligence organization—and for good reason. Intelligence agencies have consistently abused the trust placed in them and their secret powers, and arcane crafts have been missed by ambitious and zealous Cold Warriors in every administration since Truman's. From highlighting and analyzing the patterns, Johnson makes a real contribution by pointing out concretely and practically how the overseeing of intelligence can be reformed—and the necessity of doing so in order to preserve capabilities whose true value is known only to a small number of government officials. In this respect, *America's Secret Power* picks up where former director of Central Intelligence Stansfield Turner's important book *Secrecy and Democracy* (1985) leaves off. Taken together, the two books contribute a realistic basis from which professors and students today can effectively debate the role of intelligence as a public policy issue without having to feel handicapped by lack of access to government secrets.

The importance of Johnson's contribution to the debate about the future of U.S. intelligence agencies cannot be stressed enough. While there are many books today about the process of intelligence—what it is, how it's done, and by whom—most do not take on the burden of proving that secret services can function in ways consistent with U.S. democracy. But this is now critical to do, especially considering the period ahead.

The traditional argument used to justify the existence of the CIA—and the rest of the U.S. intelligence community, for that matter—is the need to have such a capability for national security. But in an era when the threats and adversaries of the Cold War are rapidly disappearing, *national security* is apt to lose its magic to unlock scarce funds or permit highly placed individuals to operate without much accountability. Implicit in this situation and in the argument of *America's Secret Power* is the need for the CIA to play by the rules, accept congressional overseeing, and train its operatives to undertake only activities consistent with U.S. law and democratic values.

ALLAN E. GOODMAN

Georgetown University

The Liberals and J. Edgar Hoover: The Rise and Fall of a Domestic Intelligence State. By William W. Keller. Princeton: Princeton University Press, 1989. 212p. $25.00.

The Boss: J. Edgar Hoover and the Great American Inquisition. By Athan G. Theoharis and John Stuart Cox. Philadelphia: Temple University Press, 1988. 474p. $27.95.

Both of the works reviewed here are concerned with civil liberties in the United States and the threat that an overzealous domestic intelligence system (namely, the FBI) poses to the security of those liberties. Still, *The Boss* and *The Liberals and J. Edgar Hoover* employ very different methodologies and adopt widely divergent—indeed, conflicting—perspectives.

The basic thesis of *The Boss* is well known: J. Edgar Hoover grossly, shockingly, and persistently abused his power throughout the forty-eight years that he served as director of the FBI. Although their argument is not new, historian Athan G. Theoharis and freelance writer John Stuart Cox have made a major contribution to the knowledge and understanding of Hoover's activities and have presented it in a highly coherent and readable way. With bureau files obtained through the Freedom of Information Act and interviews with Hoover's family members and professional associates, the authors have managed to substantiate much that was previously only suspected regarding the extent to which the Boss, in a Machiavellian quest to expand his own power, manipulated U.S. politics for nearly half a century. Hoover's career embodies politics at its ugliest. He not only compiled files on radical activists (always the object of his hatred and fear) but gathered information regarding the private lives and "immoral behavior" of members of Congress and presidents. He thereby made his position secure: no president would dare to fire him and no member of Congress would deny funds to the bureau. He also authorized illegal break-ins and electronic eavesdropping. Always a virulent anticommunist, during the 1940s and early 1950s he worked closely with the House Un-American Activities Committee and secretly assisted Senator Joseph McCarthy in his campaign to purge alleged communists from the national government. McCarthyism, according to Theoharis and Cox, would have been more accurately named Hooverism. During the last decade of his life he refused to use his authority to enforce civil rights, compiled a massive file on the sexual activities of both John F. Kennedy and Martin Luther King, Jr. and assisted Attorney General John Mitchell in a campaign to purge the Supreme Court of its liberal members, including Justice Abe Fortas.

Theoharis and Cox attribute the expansion of the power of the FBI to the determination, deviousness, and uncanny administrative abilities of its director, while William W. Keller, in *The Liberals and J. Edgar Hoover*, places the responsibility squarely on the liberal community for what he characterizes as the FBI's transformation from a domestic intelligence bureau with clearly limited functions in 1950 to an independent security state with a high degree of autonomy and insularity from other sectors of the government and from the public by the early 1970s. Keller (a senior analyst in the Program in International Security and Commerce) contends that "as the cold war heated up in the immediate postwar period, the dominant strain of American liberalism" became resolutely anticommunist (p. 36). In an attempt to remove the issue of communist subversion from the political arena, liberals turned to the FBI. He argues, moreover, that during the 1960s liberals contributed to the expanding power of the FBI by supporting its efforts to eradicate the Ku Klux Klan. (It investigated a series of Klan murders and the White Hate Cointelpro—the first instance in which the FBI moved against a group that did not have ties to a foreign government.)

Theoharis and Cox have provided a wealth of information about the life and career of J. Edgar Hoover. Their approach is primarily descriptive. Keller's work is more theoretical and analytical. Moreover, Keller provides a model of internal security in order to explain the growth of the FBI. The form that Keller's work takes is, consequently, more akin to political science. Nevertheless, if the goal is to understand the nature of power—its dimensions and the potential for corruption in politics—*The Boss* is clearly the more valuable of the two works. Beside Theoharis and Cox's meticulous and detailed examination of Hoover's exploits, Keller's argument that the liberals contributed to the power of the FBI appears insubstantial and largely beside the point. Moreover, Keller obfuscates his argument by using the word *liberal* to refer to both the theory of the modern liberal state with limited powers (a theory to which contemporary U.S. conservatives subscribe) and politicians such as Hubert Humphrey and John F. Kennedy. It is not clear whether Keller's model has any predictive value or whether it is merely a vehicle for castigating the liberals of the 1940s through the 1960s and by implication the card-carrying members of the ACLU today. The reader's suspicion that the latter is the case may be aroused by Keller's assertion that a small body of literature that has appeared during the last 10 years "attempts to demonstrate that the means and ends of McCarthyism were adopted by FBI bureaucrats and their superiors in the Justice Department and the White House. . . . But much of [this literature] is marred by a tendentious spirit, a fixation on facts and exposé" (p. 34).

SUE DAVIS

University of Delaware

The Environmental Protection Agency: Asking the Wrong Questions. Mark K. Landy, Marc J. Roberts, and Stephen R. Thomas. New York: Oxford University Press, 1990. 309p. $29.95.

Political scientists have done extensive theoretical and empirical work to explain why some groups or interests get their way in policy decisions and others do not. They have done little work, however, to explain why policy decisions are sometimes based on accurate information and coherent understanding of issues and other times are based on confusions, misconceptions, or simple ignorance. This profound book investigates the administrative performance of the U.S. Environmental Protection Agency (EPA) to address the latter question. It is a study of administrative and, more broadly, democratic deliberation. As such it joins the work of Bessette, Maass, Mansbridge, Muir, Tulis, and others in defining an important new agenda for research on public policy making.

The main research reported in the book consists of five case studies of EPA's decisions and role in environmental policy making during the Carter administration. The cases concern the revision of the ozone standard, the regulations implementing the Resource Conservation and Recovery Act, the drafting and adoption of the superfund bill, the development of a cancer policy by the Interagency Regulatory Liaison Group, and the enforcement of the Clean Air Act with respect to the steel industry. There is also a brief examination of the EPA's performance during the Reagan administration.

The objective of the research is to evaluate and explain the quality of EPA's internal deliberations as well as the agency's effect on the broader process of deliberation by politicians and the public. To orient the analysis, the authors outline a framework for understanding the relevant aspects of administrative performance. They hold that an administrative agency must discern and respect the technical merits of issues; educate the public about both the technical and the moral questions that arise in decisions; respond to interests in a manner that helps sustain a stable governing coalition; and build the institutional capacity needed to perform these functions. Achieving these outcomes, they argue, depends on establishing constructive conversation, integrating competing perspectives, maintaining strong relationships of accountability, and formulating explicit strategy.

To develop the case studies, the authors did extensive interviewing of government officials and other participants in environmental policy making. In particular, through the assistance of the Carter administration's EPA administrator Douglas M. Costle (who nevertheless takes his lumps in the study), they had extraordinary access to EPA officials—many of whom presented themselves for day-long interviews at the Harvard School of Public Health. The analysis proceeds from exceptionally thorough and informed narratives of the cases. More important, the case studies keep a sharp focus on how policy makers posed questions, used information, and responded to the realities of environmental problems. This study is a model of skillful case study research on the deliberative dimension of policy making.

For the most part, Landy, Roberts, and Thomas find EPA's deliberative performance to have been unsatisfactory. The Carter EPA, they argue, failed to explain that the genuine issue in the revision of the ozone standard was not, as Congress and the public supposed, the "safe" level of ozone pollution but rather what health effects of such pollution are worth preventing at what cost. The agency neither geared up adequately to develop credible hazardous waste regulations expeditiously nor asked Congress to revise an impossible statutory timetable for implementing the Resource Conservation and Recovery Act; it missed the deadlines by several years and still rushed to adopt hastily considered regulations. In the development of the superfund legislation, the EPA encouraged an alarmist public and congressional response, neglected to raise crucial questions about priorities, and promoted a funding scheme that misled the public about who would bear the costs of cleanup. The interagency committee on cancer policy omitted to enlighten the public on the nature of risk management and avoided any agreement that genuinely constrained the participating agencies.

At a more general level, the main failure of EPA was that it adopted a mission—protecting health—that exaggerated the health consequences of pollution and lost sight of more important consequences for the quality of life. Consciously chosen by Costle as the centerpiece of the agency's political strategy, this mission has often militated against putting environmental problems in a sensible perspective. The principal bright spots in EPA performance occurred in the middle and later years of the Reagan administration, when administrators William Ruckelshaus and Lee Thomas struggled to overcome the disastrous legacy of Anne Gorsuch and explicitly sought to improve public understanding of environmental issues.

Some readers will be tempted to dismiss the book as hortatory: it *is* partly hortatory but should not be dismissed. The authors assert that executive branch officials have a responsibility to preserve and promote constitutional democracy, especially by educating the public about the technical and moral aspects of policy choices. A principal burden of the cases, accordingly, is to demonstrate the opportunities that exist for such leadership and the harm that occurs when leaders fail to respond. This is a kind of moralizing. Because political executives always make some effort to satisfy widely held expectations for their performance, however, to set forth and defend an enlarged view of their responsibility is by no means irrelevant.

A more difficult question is whether the book holds administrative leaders and the political system to feasible standards of deliberative performance. For one thing, much of the distortion and hysteria in environmental debate results from the fact that political conflict in the United States is not largely confined to elections, as in a parliamentary system; intense conflict extends into policy making itself. Participants in such policy making must adopt a competitive orientation to debate, sacrificing a good deal of its coherence and intellectual honesty, precisely because they care about the outcomes. Thus, one can wonder whether the authors propose too elevated an ideal of policy deliberation or rely too much on changing administrators' sense of their responsibilities to achieve that ideal.

It is a question worthy of debate. Indeed, to the extent that this path-breaking study succeeds in directing greater research attention to the processes and conditions of effective deliberation, Landy, Roberts, and Thomas will have performed an enormous service.

PAUL J. QUIRK

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Homelessness amid Affluence: Structure and Paradox in the American Political Economy. By Michael H. Lang. New York: Praeger, 1989. 236p. $42.95.

Down and Out in America: The Origins of Homelessness. By Peter H. Rossi. Chicago: University of Chicago Press, 1989. 260p. $15.95.

A friend, recently returned from a visit to San Francisco, expressed his dismay about conditions there. He had lived in the marina district during the 1970s and he was alarmed by the destruction caused in that neighborhood by the recent bay area earthquake. His main outrage, however, was reserved for the plight of numerous street people and homeless he encountered during his daily walks. His memories of San Francisco as a diverse, exciting, and lovely city were shattered by the realities of the large number of homeless people there.

He is, perhaps, naive. He lives in an affluent suburban neighborhood and escapes visual contact with the large number of homeless people in the Phoenix metropolitan area. Nonetheless, his shock and dismay are genuine. A kind and gentle person, he was unsettled by the plight of the homeless, especially the numbers of women and children among them.

As we discussed the enormity of this problem, he asked, "Is this a common problem in all cities?" and "What are we doing about it?" The answers, I told him, were *yes* and *not much.* I am sure that our brief discussion did little to assure him or smooth his sense of outrage.

Like my friend's encounter with homelessness in San Francisco, reading the two present books is not an uplifting experience. There are at least 250 thousand and perhaps as many as three million people without shelter in the United States. There may be, as Rossi argues, another four to seven million persons so poor that unless conditions change for the better soon, they could join the ranks of the homeless. Not only are the numbers scandalous; but as Rossi and Lang show, the composition is much different from what it was in the past. There are many women and children among the homeless: the cliché of the skid row bum is hopelessly outdated.

Lang and Rossi, in somewhat different ways, both examine the origins and causes of contemporary homelessness, policy failure regarding the homeless, and possible reforms in dealing with them.

Lang, leaning toward the approach of the structuralist and Marxist, finds the root causes of homelessness in the free market structure of our economy and the extension of the inherent contractions of the market structure into the policy system. The intermediate causes—lack of affordable shelter, changes in family structure, reduction in federally supported social programs, welfare reform, racial and social discrimination, substance abuse, and changes in the structure of the economy—have all intensified poverty.

Rossi, more a pluralist, considers reform of shelter and poverty policies in the United States in his concluding chapters. His analysis of the causes of current homelessness and poverty is similar to Lang's list of intermediate causes and includes lack of housing, market failure, changes in labor market, limits of kinship obligation, and erosion of public welfare benefits. Rossi emphasizes homelessness as an aggravated condition of the more widespread problem of extreme poverty.

Lang says that effective (shelter and housing) policies are seldom enacted due to the array of interests against such reform. He suggests that other "well regulated market societies" such as Sweden have ensured an evener distribution of wealth while maintaining private property and democratic institutions. He asks, Why has it been so difficult for us to do the same? He answers the question by examining trends in the urban policy process, focusing on urban categorical grants and special revenue sharing. His historical-structural analysis, borrowing from both elitist and structuralist approaches, demonstrates that the fractionated policy context promotes strong interests and policy drift. Lang's book is based on an extensive review of the literature on housing and homelessless. About half of the book assesses the rare successes and notable failures of policy initiatives to provide low-cost housing and to assist the homeless.

Rossi started out to write a book entirely about the homeless; but as he examined the literature on the homeless, it became apparent that it was greater than the issue of shelter: it was about the more prevalent problem of poverty in the United States. His book, however, does focus on the homeless for two reasons. First, there is much more information about the homeless than about the larger population of the extremely poor. Second, the homeless, as a highly recognized segment of the population of poverty, have been acknowledged by officials and the media as well as extensively studied by academics.

Most of the original empirical work of the book comes from a 1985–86 survey of the homeless of Chicago conducted by Rossi. This information is used to illustrate the complexity of homelessness as a policy issue. Rossi does a good job (as does Lang) in summarizing a vast amount of literature. The literature Rossi uses tends to be more empirically based and more focused on poverty than that summarized by Lang, which deals more with policy failure.

Chapters 4, 5, and 6 of the Rossi book present the findings of his survey under the titles "The Condition of the Homeless and the Extremely Poor," "The Demography of Homelessness and Extreme Poverty," and "Vulnerability to Homelessness." Rossi's discussion of the last topic is especially important because he suggests that a large population of U.S. citizens—perhaps as many as seven million, could easily become homeless. The concept of the vulnerability of the extremely poor to homelessness is the major contribution of his book.

Rossi offers speculations about short-term and long-term remedies. Short-term remedies, such as making sure that the poor receive benefits to which they are entitled, do not require drastic overhaul of our institutional structures. Rossi, much like Lang, sees the need for major long-term structural reforms in the labor market, housing market, and welfare programs. He hints at—but does not prescribe as strongly as Lang does—overhaul of our policy system. Essentially, he argues for the establishment of a national social welfare system for which he suggests there is considerable public support.

Lang's concluding chapters offer prescriptions for reforms and an assessment of achieving them. Lang argues for a "national right to housing" supported by a national policy like Britain's Housing Act (Homeless Persons Act). He says we need a national policy because pursuit of similar, individual policies on a state-by-state basis could encounter policy fragmentation, which favors the status quo. He concludes that most interests, including those of the average citizen, would oppose a "national fair share low cost housing program." It is unlikely, Lang maintains, that the United States will embrace the national planning system he thinks is needed to ensure successful housing policy unless there is a major business crisis of the magnitude of the Great Depression.

These are both excellent books. Although they are highly speculative, the speculations are supported by the careful scholarship of the authors. They inform both the academic and the policy debates about homelessness. They are normative in intent, content, and conclusions.

Although the authors use different approaches and methods, there are striking similarities. Lang and Rossi cover overlapping bodies of literature, and it is not surprising that they see comparable sources for poverty and homelessness. Rossi's more empirical analysis makes good use of Chicago as a case study. Lang's analysis of policy failure lays a good foundation for his speculations about possible reform. Lang does not think short-term reforms will do much; Rossi finds them useful. Both think that long-term, far-reaching structural reform is needed with Lang suggesting more radical changes in political structures. Neither is confident that the needed reforms will happen, although Rossi is the more confident of the two. As noted earlier, these realistic, hard-hitting analyses are not uplifting—but neither is homelessness.

LOUIS F. WESCHLER

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Constitutional Faith. By Sanford Levinson. Princeton: Princeton University Press, 1988. 250p. $12.95 paper.

Unlike most contributors to normative constitutional debates, Sanford Levinson, in *Constitutional Faith,* is not trying to nail his own 96 theses to the door of the U.S. Supreme Court. Rather, his concern is to explore the intellectual problems facing any such assertions of faith. His essays will prompt most readers to perceive some of the most familiar of those problems afresh and push many to reflect on other problems that we usually ignore. What role does the Constitution really play in promoting a measure of unity in such a richly variegated nation? What should we make of the brief loyalty oaths that are tucked into so many public rituals, such as officeholding, registering to vote, even getting a passport? What work should be done in the law schools that certify the priesthood of the nation's constitutional faith while promising to expose all frauds? In the end, if we had it to do all over again, would we actually sign this Constitution?

Judging by the early returns, Levinson's eloquent and engaging probes of these questions are already achieving their aim of inspiring or provoking other scholars to give more definitive answers to his queries than he himself attempts. He is most successful in sharpening our sense of how much every effort to read the Constitution carries with it some preliminary implicit theory of what the Constitution is and why it may be important (or else we would never bother to read it). These different implicit theories often then generate significantly different understandings. He is equally compelling in suggesting how greatly U.S. acceptance of the Constitution rests essentially on Human reasons, on an awareness that it would be dreadfully inconvenient to try to jettison or greatly transform it. Few are genuinely prepared to defend as settled beliefs the common claims that this frame for government and public life is really most choice-worthy. If we acknowledge Levinson's successes in making these points, we are forced to try to work out again and better what we are looking for when we turn to the amended text of 1787 and the traditions that have grown around it.

Levinson can provoke, as well as inspire, responses, however; for in this book he gives us only the briefest sketches of his own answers and that for only some of the questions. In a chapter extending his well-known taxonomy of "Protestant" and "Catholic" approaches to constitutional interpretation, for example, he lets it be known along the way that he is "Protestant" in wanting all to be interpreters but "Catholic" in wanting subsequent traditions to be grist for interpretation as well as the original text. But just why he is "Protestant-Catholic" in this way is never very directly elaborated. He also touches on various important empirical claims, such as the alleged success of constitutionalism in serving as the nation's unifying "civil religion"; but it is no part of his inquiry to try systematically to see whether these empirical claims, which he often appears to accept, are justified.

On the empirical side, Levinson may simply be staying within his considerable professional strengths, although perhaps he should then be more cautious about claiming a great deal for constitutionalism's role in legitimating U.S. governance. On the normative side, in contrast, one wishes he had been more bold. It may simply be that Levinson is unsure of his normative answers to his questions of constitutional faith. That is fair enough, though we might still benefit from his attempts to argue for them. But he does occasionally imply a more disturbing reason for his silences. He suggests at one point, while asking what we take the Constitution to be, that "it is doubtful that logical argumentation plays a crucial role" in making these determinations (p. 37). Although it is probably true empirically that we begin examining the Constitution with unargued, inherited notions of it, it does not seem true that we are stuck with those notions—that argumentation, including logical argumentation, cannot dramatically alter our views over time.

If the unargued quality of our original notions is taken to indicate that rational debate over first questions is fruitless, Levinson's failure to argue for his fundamental answers may be understandable. But it is a failure based on a pessimism more thoroughgoing than is required by any of the admittedly daunting problems he raises. And it is in fact unlikely that Levinson really is so gloomy about the potential of rational human inquiry, since he writes here and elsewhere so much to spark it and advance it. It is more likely that immersed like all constitutional scholars in examples of failed attempts, Levinson is tremendously impressed with the difficulties of arguing decisively and effectively for one's normative constitutional beliefs. So he ventures in that direction, but only cautiously. *Constitutional Faith* suggests that both he and his readers might benefit if he would choose to venture further.

ROGERS M. SMITH

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Minutes to Midnight. Nuclear Weapons Protest in America. By Frances B. McCrea and Gerald E. Markle. Newbury Park, CA: Sage, 1989. 200p. $27.95 cloth, $14.95 paper.

The year 1989 will enter the history books as one of the most remarkable years of the century. Considering the revolution in Eastern Europe and the end of the Cold War, it seems almost hard to believe today that the decade was born under the dramatic threat of nuclear war. In response to the withering of geopolitical détente after the Soviet Afghanistan invasion in 1979 and the military buildup by the incoming Reagan administration, the broadest mass movements in recent history emerged in Europe and the United States, demanding an end to the nuclear arms race. Whereas the European peace movements targeted the stationing of a new generation of middle-range nuclear weapons by NATO and, in effect, advocated unilateral and total disarmament, the emerging U.S. movement centered around the more moderate Freeze campaign, which called for a bilateral halt to the testing, production, and deployment of new nuclear weapons.

Contrary to its political significance, the rise and fall of the Freeze has so far received only scant scholarly attention; thus, Frances B. McCrea and Gerald E. Markle's *Minutes to Midnight* must be considered a welcome contribution. This book not only focuses on the Freeze but delivers a brief history of nuclear weapons protest in the United States, including the Atomic Scientists movement of the 1940s and the Ban the Bomb movement of the late 1950s and early 1960s. The strength of the book is that it provides a concise and readable historical overview of these movements, especially the Freeze. Its weakness is that it fails to develop a convincing theoretical frame to structure the historical narrative. Moreover, the chapters on the Atomic Scientists and Ban the Bomb movements borrow too much from already published sources and have little new to say. Finally, the account of the Freeze campaign is somewhat biased by the authors' obvious disdain of the "legalistic" strategy pursued by the Freeze; and they curiously ignore the parallel existence of a more radical direct action wing in the U.S. peace movement of the 1980s.

In theoretical regard, the authors claim to synthesize the dominant *resource mobilization theory* (RMT) in social movement research with micro-level *social constructionism* and macro-level *new class theory*. They rightly see that RMT ignores the symbolic aspects of social movements, such as *claims making* and the launching of new collective problem definitions. Even though it failed to realize its more tangible goal of getting a strong nuclear freeze resolution passed in Congress, the Freeze's real success was to arouse the public to the salience of the nucelar issue. However, instead of bringing the symbolic dimension of movements back in under the obscure label of social constructionism, the authors could have indicated that the eminent collective behavior theory of Ralph Turner and Lewis Killian has addressed this crucial aspect of social movements under the label of *emergent norms* for almost thirty years.

Resource mobilization theory not only downplays the symbolic aspects of movements but also fails to connect the rise and fall of social movements with large-scale processes of social change and modernization. This shortcoming of RMT is rightly identified by the authors, but their attempt to fill the macrogap by new class theory does not convince. Certainly, intellectuals played a prominent role in the various peace campaigns of the postwar period. But already in RMT, with its key emphasis on moral entrepreneurship, there is ample consideration for the role of intellectuals in social movements. The reference to new class theory is also substantially misleading because the new risk movements since the 1970s, such as the environmental, anti–nuclear energy, and peace movements, are characterized exactly by their nonclass agenda, addressing, instead, collective threats to society as a whole.

In short, while the authors rightly identify certain shortcomings in the dominant resource mobilization paradigm, their attempt to deliver a "concomitant analysis of micro-level claims making, organizational level resource analysis, and macro-level class analysis" (p. 20) is flawed. Moreover, the theoretical analysis is disconnected from the historical narrative: for instance, the occasional identification of particular intellectuals in the various peace campaigns does not yet substantiate their new class concept, which is anyway implausible.

In empirical regard, the account of the peace movement of the 1980s, especially the Freeze, is not always well balanced. The Freeze was only one—albeit an important—strand in a much broader movement that also comprised direct action groups, such as the American Peace Test, which linked the call for disarmament with more general issues of social justice and change. This neglect is even more surprising because the authors reproach the Freeze with pursuing an overly "legalistic" strategy. The authors attribute the rapid decline of the Freeze since 1984 to "co-optation" by the Democratic Party, which allegedly drowned the issue in the muddy waters of electoral politics. The authors operate on the hidden assumption that a noninstitutional and uncompromising movement strategy would have been more promising. How do they know? Indulging in romantic myths of insurgence conceals the fact that the Freeze has been one of the most successful social movements ever to occur in the United States. The authors mention, but fail to appreciate, the fact that the Freeze mobilized millions of people both at the grass-roots and national levels and was endorsed by more than 20 state legislatures, numerous city and county councils, town meetings, the Catholic Church, and many national labor unions—and this in the conservative Reagan era. The really interesting question is not the decline of the Freeze but its meteoric rise in a time unfavorable to Left-liberal causes.

In sum, for those who want a brief and descriptive overview of the history of nuclear weapons protest in the United States, this book is interesting reading. Those, however, who expect a more sophisticated integration of theory and empirical analysis will be disappointed throughout. A comprehensive and analytically satisfying history of the U.S. anti-nuclear weapons movement has yet to be written.

CHRISTIAN JOPPKE

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Domestic Policy and Ideology: Presidents and the American State, 1964-1987. By David McKay. New York: Cambridge University Press, 1989. 223p. $34.50.

David McKay, a British political scientist, takes an optimistic view of U.S. presidents as domestic policy makers. McKay is critical of the bargaining model of the presidency, first established by Richard E. Neustadt, for assuming that presidents must adapt to a relatively fixed political environment. This model, he argues, underestimates the capacity of presidents to alter their environment, especially through agenda setting. In domestic policy, presidents can pursue their own preferences, formulate coherent policies, and seize opportunities for innovation not available to other institutions in a fragmented political order.

Where followers of Neustadt have stressed presidential power, McKay seeks to give greater weight to presidential ideology. He complains of the prevailing paradigm in presidency studies that it "leads inquiry away from treating presidential ideology as an independent influence. A review of the leading texts on the institution reveals almost no reference to ideology in the indexes" (p. 4). In McKay's view, the White House typically approaches domestic policy less as a domain of politically expedient bargaining than as a field for the expression of presidential preferences.

Domestic Policy and Ideology selects two areas of domestic policy—welfare reform and intergovernmental relations—and traces their fate through the administrations of Presidents Johnson, Nixon, Carter, and Reagan. Drawing from secondary sources and from archival material and interviews, McKay examines policy origins, coherence, and adaptation in each of these four presidencies. The conclusions he reaches are provocative. As the U.S. polity has become more fragmented, he argues, presidents have actually become more autonomous from parties and other outside forces. Through internalized policy processes, they now exercise "a high degree of control over initial program design" (p. 209). They also have a superior capacity to formulate coherent policies even if these policies must later be compromised in the course of legislation and implementation. Presidential capacities for administrative adaptation of domestic policy have also grown, as dramatically demonstrated under President Reagan. McKay believes that the success or failure of a contemporary president in domestic policy depends, in the end, on leadership skills. "Potentially," he writes, "a president able to combine leadership, policy coherence and a carefully worked out adaptation strategy should be able to achieve major successes during his first term" (p. 213).

Although the argument of *Domestic Policy and Ideology* is intriguing and important, the historical materials presented by McKay do not sustain such an optimistic conclusion. The case studies he presents of Johnson, Nixon, Carter, and Reagan are too cursory to be fully persuasive. Thus, McKay insists that in each of these administrations domestic policies were formulated through internal processes, with forces outside the executive office of the president playing a minor role. Yet he does not look closely either at the sources of the ideas utilized by White House policy makers or at the political forces backing alternative options. When outside actors play a signal role, McKay treats their involvement as idiosyncratic. Walter Reuther's suggesting the Model Cities program to the Johnson White House is explained away as the action of "an ambitious individual" (p. 42). McKay fails to mention Reuther's prominent place in both the liberal-labor coalition and the civil rights movement.

McKay does not make a strong case for the influence of presidential ideology on domestic agendas. Indeed, with the exception of Ronald Reagan, the case studies paint a picture of nonideological presidents. "Documents from the Nixon archive," McKay notes, "confirm that the President's support [for welfare reform] was motivated not so much by any substantive interest in the area, but by a desire—even an obsession—to make some sort of an impression on domestic policy" (p. 77). Turning to Carter, McKay finds a similar story: "Even more than Nixon, his agenda lacked ideological unity. He had no clear vision in domestic policy" (p. 137). Finally, McKay does not have much historical evidence, apart from Johnson's Great Society, to support his claim of potential presidential successes in domestic policy. Not only do Nixon and Carter fail with welfare reform—Reagan's New Federalism initiative of 1982 is equally a flop.

Domestic Policy and Ideology is valuable for its challenge to the Neustadtian paradigm and for its insistence that we look closely at the ideological preferences and internal policy processes at work when presidents formulate domestic policy. Yet the history it relates does not fit so neatly into McKay's exclusive focus on the president and his immediate advisors. Failures in domestic policy may stem, in part, from the inadequacies of a president's leadership, as McKay suggests. He has neglected, however, to give sufficient weight to the broader political and economic forces that influence the formulation, as well as the fate, of domestic policy in the United States.

BRUCE MIROFF

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Bound to Lead: The Changing Nature of American Power. By Joseph S. Nye, Jr. New York: Basic Books, 1990. 370p. $19.95.

Joseph Nye is rightly respected for his contributions both to scholarship and to government service. His new book—a vigorous and generally persuasive refutation of stronger forms of the "declinist" perspective associated with Paul Kennedy—deserves widespread attention.

Much of his thesis that the demise of U.S. hegemony has been greatly exaggerated provokes a certain sense of deja vu. I encounter familiar arguments, well developed—that U.S. hegemony clearly surpassed that attributed to Britain in an earlier era; that the decline in U.S. hegemony is sharp only as measured in the first two decades of the post-1945 era, from a peak artificially exaggerated by the temporary exhaustion of everyone else by World War II; that power is properly conceptualized as control over outcomes (in which the U.S. continues to do well), as contrasted with mere share of resources; that the force of U.S. commercial and popular culture is a key component in U.S. control over outcomes; and that hegemony is not, as often imagined, primarily a matter of the hegemon's being exploited in the provision of collective goods to smaller powers. The arguments are brilliant, important, logically compelling, and empirically sound.

But he extends these arguments and adds much more. Distinguishing between the effects of economic and military hegemony, he contends that U.S. dominence of the world economy had declined markedly by the end of the 1950s, largely in consequence of a deliberate policy of rebuilding the strength of U.S. allies. Thus, if the greatest decline in economic dominance occurred that long ago and the basic features of the liberal world economic order have nevertheless been maintained, contemporary fears about conditions "after hegemony" need not be so great. In the military sphere, he says, the use and success of U.S. military threats has been exaggerated at all times, limited by the global structure of bipolarity, especially in nuclear weapons. In this I tend to agree but find, rather than a very modest utility of nuclear threats throughout the post–World War II era, a distinct decline after the early 1960s as the Soviet Union achieved a clear second-strike capability.

After dismissing alarmist interpretations of the past, Nye turns to a carefully balanced discussion of implications for the future. He considers various diagnoses of domestic political and economic "sclerosis" and, while not dismissing them, notes also the great strengths of the U.S. system. Not least of these is its flexibility and creativity, despite—or very likely because of—the weakness of its state. At the same time he affirms a need for higher taxes, in this most lightly taxed of industrial countries, to support improvements in infrastructure and education and to avert the continuing piling-up of foreign debt that will ultimately become a drag on the economy. He also says sensible things about the need to maintain an open world economy and to strengthen international institutions.

With the collapse of the Soviet alliance, greater now than when he wrote, the book's major arguments seem even more persuasive. The Soviet Union's nuclear deterrent covering its own territory remains intact. But the wider decline of Soviet military power implies, of course, that the burden of military spending on the U.S. economy relative to that on the economies of its major economic competitors can now decrease. Whether the opportunities of continuing semi-hegemony-cum-interdependence will be seized sufficiently is not clear either to me or to the author. Perhaps he is too optimistic.

Nye's vision is primarily that of a liberal internationalist cognizant of the utility of power. Both as a piece of academic theory (written forcefully and gracefully for a wide audience) and as a guide to policy making (especially if Nye ever gets that chance again) we could do much worse.

BRUCE RUSSETT

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The Politics of Earthquake Prediction. By Richard Stuart Olson. Princeton: Princeton University Press, 1990. $19.95.

The politics of science has become the focus of a growing literature, inspired, perhaps, by the fact that more and more research is directed toward short-term technological applications. Moreover, controversies over technology have proliferated, engaging scientists in debates over, for example, the risk of nuclear power or the dangers of toxic wastes. Such disputes, focusing on the prediction and evaluation of risks, offer unique opportunities to study the politics of science and technology and the conflict between scientific goals and political expectations.

This is the context of *The Politics of Earthquake Prediction*, a detailed case study of a scientific controversy over a methodology of forecasting earthquakes. In 1976, Dr. Brian Brady and Dr. William Spence developed a theory of earthquake prediction that challenged conventional wisdom in the field of seismology. Based on their theory, they predicted that there would be a devastating earthquake in Lima, the capital of Peru, in 1981. Not surprisingly, the prediction generated a political and media circus. Though the Brady-Spence method of prediction was controversial within the scientific community, it aroused understandable concern in a country like Peru, with its history of traumatic natural disasters. A major earthquake in Lima, a city of about five million people, would, according to a

United Nations estimate, kill up to four hundred thousand people, and destroy 50% of private dwellings and 10% of industrial buildings. Even the prediction of such a catastrophe had economic and social consequences, effecting land values and investment plans in the area where the earthquake was expected and certainly arousing public fear.

The story, as developed by Richard Olson and his colleagues, suggests the sensitive international political relationships involved as U.S. scientists predicted a disaster in a foreign country. They show how government agencies, civil defense forces, the media, and scientists all became involved in efforts to "do the right thing." They suggest the complexity of civil defense preparation, the politics of organizing medical resources, the concern about the media and their ability to generate panic. The problems were compounded by the changes in the Peruvian government from military to civilian control. As press control lifted, the Peruvian media turned the earthquake prediction into a public issue. At the same time, the U.S. press announced, "Great Quake Predicted for Peru."

When 1981 arrived, Lima citizens were deluged with advice about "what to do." Tourism declined. Civil defense agencies made elaborate plans. The population braced for disaster. Well, nothing happened; so Brady, after all this, announced in July 1981: "I believe that my prediction . . . is incorrect." He went back to his former work in the Bureau of Mines.

The story is interesting, and Olson and his colleagues develop it with rich documentation. But their use of the case to explore how the scientific community responds to challenges to orthodox methods seems bizarre and in a way makes them miss the point. They discover, apparently to their surprise, that "extra-scientific" factors—the media, the interests of bureaucrats, the political situation, the stakes of other scientists—all influence scientific controversies. Yet how could it be otherwise? They mention, yet minimize the seriousness of, the problem with inconclusive predictions when these have such major social repercussions.

Given the nature and implications of an earthquake prediction in a major city, a political response is hardly surprising. Earthquake prediction is hardly an academic exercise in methodology. Given the profound social consequences of the prediction itself, why shouldn't the media cover the issue as a political and public issue? Why shouldn't politicians be involved? The purpose of the book, as stated in the introduction, is "a detailed dissection of a major scientific controversy." But I found it strange to define earthquake prediction—especially one with such devastating consequences—as a scientific controversy. Perhaps it reflects the persistent tendency to depoliticize issues by labeling them as science. Yet the case itself suggests that labeling political issues as science can also be a way to limit public involvement in matters that clearly need public scrutiny and to avoid discussions of responsibility.

This story of the politics of prediction is a good one, well written, even gripping. But treating the dispute as a scientific controversy tells us little about the relationship between politics and science beyond the hubris, and demonstrates the irresponsibility of a few politically naive individuals.

DOROTHY NELKIN

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United States Railroad Policy: Uncle Sam at the Throttle. By Jeffrey Orenstein. Chicago: Nelson-Hall, 1990. $23.95.

I found the title of this book picturesque but a bit misleading. The history of Uncle Sam's railroad policy interventions has been one of no hands on the throttle and both hands on the brake—today, perhaps, one hand on each. In this interesting and readable account Orenstein describes all of these different roles for Uncle Sam the engineer and how each role is affected by the political economy of the United States. The title is most appropriate for Orenstein's ultimate thesis that Uncle Sam should be at the throttle, that U.S. political involvement in the operation of the railroads is both necessary and justifiable but currently incomplete.

Orenstein's analysis offers several rewards to students of public policy. First, he sets an admirable example in being explicit about his own assumptions. Since all policy studies are inevitably affected by the author's own set of biases, describing the color of the lenses up front enables the reader better to ascertain objectivity. Second, Orenstein supplies a useful typology of political economies with which to compare government intervention. He uses different aspects and periods of railroad policy to examine market allocation, government enhancement, quasi-nationalization, and nationalization of economic sectors. Third, the author provides an informative historical account of railroad policy in the United States. His explanation for the competitive disadvantages of railroads that resulted from misguided regulation while other forms of transportation were receiving substantial federal subsidies (e.g., automobiles, with highways) is compelling. Finally, Orenstein soundly criticizes the lack of a comprehensive federal rail policy. The United States suffers, instead, from "a series of partial policies" (p. 116). Policy scholars will enjoy using Orenstein's analysis to buttress prominent ideas in the literature, such as Lowi's criticism of the inability of the government to plan effectively.

Readers should be aware of one aspect of Orenstein's book that may be more disturbing to political scientists than to casual readers: the theoretical argument could be more consistent and systematic. Advocating a particular role for government in railroad policy is a normative argument that does not replace the need for positive analysis. In regard to the latter, simply saying that the political economy of the country affects its railroad policy does not tell us much. Of course it has effect. But U.S. railroad policy as an entity is a troublesome dependent variable since it involves (as the author himself says) numerous shifts over time and conflicting portions at any one time. If the author wants to explain the shifts over time (from laissez-faire to regulatory in the late nineteenth century and regulatory to quasi-nationalization in the 1970s) he should be clear about which independent variables are important and how they have changed. If he is describing different portions of railroad policy (such as freight and passengers), he should compare the independent variables across cases. Instead, Orenstein uses potentially powerful independent variables mainly when they are useful. What are the relative and changing impacts of electoral politics (p. 60), national crises (p. 71), organized interests (p. 116), and dominant cultural allegiance to free enterprise (p. 78)? Each of these make interesting explanations in specific cases, but the reader may be frustrated that overall explanation of railroad policy is somewhat unsystematic, not unlike its subject.

The book is best when it concentrates on the subject at hand. The accounts of Amtrak and Conrail are succinct and informed. The author loses some of his focus when he attempts to do too much, as in defining everything from business firms to judicial review. Mentioning complex phenomena only briefly may leave specialists unsatisfied. For example, development competition between states warrants more in-depth analysis than can be provided by one paragraph (p. 35). Placing railroad policy in an overall context is a worthy ambition; but the author should be careful that he not try to reinvent the whole train, let alone one of its wheels.

Finally, while he opens himself up to criticism for his policy prescriptions, Orenstein is to be commended for offering specific recommendations. Some will surely question his use of the term *public interest*, but many of the author's proposals make sense at least to me. Orenstein's perspectives contribute to the worthiness of this book for analysts of railroad policy specifically and for students of the policy process in general.

WILLIAM R. LOWRY

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Black Politics in Conservative America. By Marcus D. Pohlmann. New York: Longman, 1990. 271p. $17.95 paper.

This discussion is in the genre of the political economy analysis. The author seeks to establish the place of black U.S. politics by analyzing the intertwine of political and economic factors. Several Marxian analytical concepts are combined in an interesting way with traditional elite analysis represented in authors like Murray Edelman and Michael Parenti and with the colonial model elaborated by Stokely Carmichael and Charles Hamilton. These theories help the author to define a U.S. class structure, complete with the obfuscations that create false consciousness; to identify an economic elite in collusion with government; and to elaborate a system of conduit colonialism that locks the middle class and poor (including virtually all blacks) out of equitable resource acquisition. As such, the approach is not new. It is a part of increased concern with the interplay of politics and economics. However, the author arranges this material in a way that provides an interesting perspective about blacks in U.S. politics.

Several aspects of this work deserve very close reading: the modification of David Easton's systems model, the description of the U.S. class structure, and the augmentation of Charles Hamilton's conduit colonial model. In regard to the systems model Pohlmann shows limitations that result because the political arena is entirely separated from its environment. He shows many circumstances in black political experience unaccounted for by the model. He notes the absence of black influence on the distribution of benefits in the postindustrial economy, for example. Since they have virtually no control of this economic sector, blacks are also powerless in influencing allocations. Yet virtually all aspects of the postindustrial sector are determined in the same environment that establishes the agenda of government. When this environment is isolated from politics, the latter is hardly revealed. Pohlmann graphically renders an alternative of the systems model that captures his point about the interdependence of the environment and the political system. Curiously, this clever point is later undermined when Pohlmann characterizes his theory as a combination of the pluralist systems and Marxian approaches.

The class structure argument closely resembles Marxian theory. However, following Murray Edelman, Pohlmann identifies where wealth *is* in society, and shows how this hardens class lines. Then he goes further than others by showing the minuscule proportion of the wealth owned by blacks. One observes, therefore, the disabled condition of blacks, who merely own residual wealth, which ultimately translates to powerlessness.

Perhaps the most challenging theory is the modified conduit system. Charles Hamilton first used this to explain how welfare transfer payments to the poor ended up as benefits to the wealthy elites (welfare beneficiaries). These wealthy landlords, physicians, and merchants controlled *inter alia* a pass-through system whereby welfare recipients paid inflated prices to elites for goods and services. Pohlmann expands this concept to include the middle and working classes, who sustain this system in exchanges that transfer their finances to the elites. The foundation is taxes paid by them to government. Through their labor these groups generate private sector, corporate profits for the beneficiaries. A cycle then ensues. The government provides public assistance to the welfare recipients, who then pay for services from the beneficiaries. The latter maintain their control by reinvesting into the private sector and by receiving direct subsidies, contracts, and interest from government. This circular system has all the reinforcements to be self-maintaining. Without major changes there is little hope for redistribution of power or resources, Pohlmann argues.

The balance of the analysis is an application of the model to black political life in several arenas—judicial, electoral, legislative, and information. This application is less successful than the theory, though some interesting and useful points and prescriptions are made. The interesting points result from analysis of new materials that other general studies have not considered. In the study of the judiciary Pohlmann considers not only cases but the criminal justice system. On elections he covers some strategies used in the post-civil rights mobilization. This is linked to the question of legislative agenda setting and participation. The information variable is treated eclectically, including education and other areas (such as the media and church) that "misocialize" the masses. The remedies proposed are sometimes novel and often appear to have little to do with the theory. Among other things diminution of the system of checks and balances is called for, as well as a kind of proportional representation. All this is said to minimize the capitalist exploitation and/or chaos in the system that keeps blacks powerless. Curiously, a major prescriptive theme is the adoption of a parliamentary system, long since noted for its capitalist exploitation. This is notwithstanding the question of how this parliamentary model would work with his multiparty system, a device proposed to accommodate more proportional group representation.

Despite the problems noted above and several distracting and unnecessary long recitations of events in black history, the theory in this analysis deserves serious consideration. It provides a direction that might be applied to the experiences of blacks as they struggle for inclusion in U.S. society and politics.

MINION K. C. MORRISON

University of Missouri, Columbia

Biting the Hand That Feeds Them: Organizing Women on Welfare at the Grass Roots Level. By Jacqueline Pope. New York: Praeger, 1989. 161p. $39.95.

Planting the Grassroots: Structuring Citizen Participation. By Steven H. Haeberle. New York: Praeger, 1989. 151p. $38.00.

These two studies of grass-roots activism display the difficulty local people have in articulating demands and sustaining pressure to try to achieve reform. Both Pope and Haeberle's works are examples of the value of in-depth, contextual research of the people and local cultures involved in grass-roots politics. Both studies display the importance of the organizational structure of government and local political climate for estimating the success of grass-roots pressures for reform in public service delivery. Pope's study is of welfare mother's organizations in New York, primarily Brooklyn, in the 1970s. Haeberle writes about the efforts by city officials in Birmingham, Alabama to increase citizen participation through neighborhood organizations. One common finding from both studies is how activists desire larger reforms than they actually demand from government officials. One power government retains even in the face of pressures from the grass roots is the preemptive agenda setting, which participants internalize by scaling down their goals to small reforms they believe might be achievable.

Biting the Hand That Feeds Them is based on interviews Pope conducted with participants in the Brooklyn Welfare Action Council (B-WAC). Pope's analysis is enriched by her observations based on her participation in welfare rights organizing in the late 1960s. B-WAC was established in 1967 by women of color who received public assistance. It was the largest single chapter in the National Welfare Rights Organization and was distinguished by the high amount of participation and leadership by welfare women in the group. Members of B-WAC knew full well the deep structures of inequality, race, and gender discrimination that kept them impoverished and dependent. Of the many everyday indignities these welfare mothers endured, particularly pernicious were the inconsiderate and belittling rules and procedures of the New York City welfare agencies. One accomplishment of these activists was to obtain less disrespectful treatment from welfare officials.

Planting the Grassroots is a study of Birmingham's unique citizen participation program. Since 1974 Birmingham has had a largely successful neighborhood-based citizen participation system, relying on 86 separate neighborhood associations, officially sponsored by the city government—this in a culture without a participatory tradition. Haeberle conducted interviews with neighborhood association presidents, analyzed both voter turnout for neighborhood association elections and demographic characteristics of neighborhoods, and examined the written record of neighborhood groups and city counsel files.

For the groups studied by both Haeberle and Pope, outside groups and activists were important in getting the organizations started and in sustaining them through difficult times. Pope reports on the important, if controversial, role played by activist nuns, priests, and VISTA volunteers in organizing and nurturing B-WAC. Haeberle documents how city officials deliberately set up and sustained the neighborhood organizations necessary for their community participation plans. Both authors refer to the mixed blessing contributions by outside activists, no matter how well intentioned, are to indigenous groups. Members can become dependent on the resources of the outside group, defer to leaders from that group, and allow other interests subtly to restrict the fledgling group's agenda and leadership pool. Yet the resources provided by outside organizations helped recreate an opportunity for otherwise underrepresented interests to articulate their demands to government officials.

For the welfare mothers in Pope's study, breaking the silence of their isolation and self-blame was an invigorating experience. Standing up to the Department gave them new feelings of self-worth and an orientation toward public assistance that stressed their right to receive adequate treatment and benefits. Many of the women active in B-WAC in the late 1960s and early 1970s are no longer on public assistance, Pope found. Although Pope recognizes the possibility that the most talented and motivated welfare mothers became active in B-WAC, she might have developed this point further.

Activists interviewed in Pope's study noted their isolation from mainstream civil rights groups. Pope acknowledges that some of this lack of coalition building stemmed from the reluctance of civil rights activists to be identified with welfare mothers. She also notes that groups such as B-WAC made strategic mistakes by distancing themselves from other local movements for social change and not engaging in coalition efforts.

These two studies contribute to the ongoing debate about the relative merits (for those seeking change) of organizing and pressuring for incremental reforms versus withdrawing their consent and standing in protest and opposition to the status quo. Haeberle notes that the citizen participation network for Birmingham legitimates official policies and moderates demands from the grass roots. At the same time, it has increased the access of otherwise less influential citizens to local policy makers and has been a proving ground for neighborhood residents to run for city and county offices. Pope deals directly with these questions, even interviewing Frances Fox Piven and Richard Cloward for her study. Although Pope is aware of the potential organizing has to diminish the power the poor have during a time of rebellion, she also found evidence that B-WAC and other groups sustained individuals during their struggles and empowered them with a sense of group solidarity. Neither Pope nor Haeberle choose sides in the organizing-versus-protesting debates.

Pope could have elaborated on the confluence of race, class, and gender inequalities in these people's lives and the subsequent difficulty in organizing this group. It also would have been useful if Pope had explained more fully what led to the demise of B-WAC. Haeberle's work is a comprehensive analysis of a citywide program for institutionalizing citizen participation (through neighborhood associations) into local politics. In a measured and balanced style, Haeberle notes the strengths and weaknesses for this officially sponsored grass-roots activism. Both studies make valuable contributions to the literature on grass-roots politics in the United States.

LAURA R. WOLIVER

University of South Carolina

Reforming the Workplace: A Study of Self-Regulation in Occupational Safety. By Joseph Rees. Philadelphia: University of Pennsylvania Press, 1988. 247p. $34.95.

In the mid 1970s, only a few years after its inception, the Occupational Safety and Health Administration faced various problems. From a future that had once appeared so promising, OSHA had fallen prey to a host of maladies, the least of which included the problems associated with nebulous standards and regulations. OSHA's inability to forecast, or account for, changes in technology resulted in an administrative environment that was not very conducive to change.

The advent of the 1980s saw a change in OSHA's policy initiatives, as strict forms of government regulation were relinquished in lieu of new patterns of workplace self-regulation. Industries with proven records of safety were given the opportunity to have more control over safety and health regulations. In *Reforming the Workplace* Joseph Rees explores the apparent success of one such program, the Cooperative Compliance Program (CCP), and discusses how this different approach to reform resulted in lower accident rates as well as higher incidence of cooperation among unions and employers.

At the outset of the study, Rees asks, "When regulatory enforcement is relaxed, will business firms behave responsibly?" (p. 1). This issue is central to Rees's conviction that industrial self-regulation can be a less stringent and more synergetic alternative to OSHA's punctilious style of legalistic enforcement. He attempts to convey the idea that the regulatory process is a nonstatist system indigenous to a broader social environment shaped in part by the constant interplay among an array of historical and modern factors. Rees believes that the development of enterprise health and safety responsibility was fostered in the California CCP model because OSHA allowed company officials to acquire more authority and autonomy. These developments were accentuated by the fact that both labor and management representatives worked to extend the social acceptance of this program among employees and administrators.

Rees indicates that the CCP was a success because accident rates were lowered and job site safety programs were fortified through the program. In addition, he notes the fact that none of the major parties took the option of withdrawing from the study (via an escape clause) as further justification of the program's worth.

This volume offers a rich, thorough discussion of the interactions associated with public and private occupational safety and health regulation. Rees's coverage of how government regulation influences the day-to-day conduct of regulated forms raises many intriguing questions as to the utility of traditional forms of regulation. Much can be learned from his discussion of power bases and organizational mandates, as well.

The insightful contributions of Rees's work are tainted only by the thinness of his research. He breaks the study down into two portions: the first looks at construction under the traditional OSHA plan, the second involves the self-regulated CCP model. One is alerted to the problems with this model from the start, however, since the author notes that "to some but not all of the participants . . . there is reason to believe that the latter half of the project (CCP) may have been less hazardous compared to the first half (OSHA) simply because of the different construction processes involved" (p. 18). Rees does not disclose why "some" saw the program as a success, while others did not. In addition, he does not reveal how many people were actually included in the category *some*. Thus, one is wary of the conceptual soundness of his study from the start.

Apart from this discrepancy, there are other instances where Rees's research techniques seem to be flawed. He notes that many of the files he relied on for data for this study have now been erased. As such, he feels that it is important to "present the details of how the research was carried out" because "the research reported here cannot be replicated" (p. 241). Unfortunately, to this end, Rees does a marginal job. In his discussion, he states that he interviewed approximately 75 individuals, but he does not state the total size of the population group.

To add to this ambiguity, he notes that out of the 75 respondents, only 15 were construction workers and adds, "Needless to say, this is not a representative sample, given that many thousands of workers were involved in CCP projects" (p. 244). Overall, too little space is devoted to a clear discussion of research methods. The book is remiss in not providing examples of survey questions or subject areas despite the author's references on several occasions to a "interview guide" or "checklist of general topics" (p. 243).

The author has many interesting premises, but the vague, inconsistent nature of his research serves to discredit the apparent success of the CCP as a whole. While Rees's work has obvious methodological shortcomings, his well-constructed narrative of the mitigating forces involved in the implementation of OSHA standards serves as an interesting contribution to the understanding of occupational safety and health.

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Foundations of American Constitutionalism. By David A. J. Richards. New York: Oxford University Press, 1989. 315p. $32.50.

The enduring authority of the U.S. Constitution in shaping U.S. values and policies is a subject of perennial interest to political scientists, as well as lawyers, judges and politicians. Although judicial controversies in the twentieth century have long since dissipated notions that the Constitution is a ruler against which statutes can be measured, the search for a definitive and persuasive explanation of the authority of the Constitution continues. The intensity of the search is heightened by the Supreme Court's potential shift on issues such as abortion and affirmative action, continuing controversy over symbolic speech and church-state issues, former attorney general Edwin Meese's advocacy of original intent, and the rejected nomination of Robert Bork. Richards offers a general political theory of constitutional law to satisfy the search. He argues that constitution making was both an interpretative and a creative act. His analysis describes the framers' reliance on, and critical interpretation of, historical events and uses this model as the standard for judging past and contemporary interpretations. The emphasis is on critical interpretation to avoid the pitfalls of excessive reliance on ancestral authority. The critical exposition of Locke's theory of the protection of inalienable human rights as the source of political legitimacy is the core of the interpretive convictions of the U.S. revolutionaries.

A brief example of the critical use of interpretative history illustrates how the method works. Questions of modern constitutionalism center on how to structure and use political power in ways that respect equal rights and further the pursuit of the public good, that is, the impartial tests of Lockean political legitimacy become justification for current political decisions. The theory has some practical difficulties. For example, Richards uses *The Federalist* number 10 to argue that federalism is the antidote for the ills of classical republicanism. By focusing on representation and modifying Locke's principle of majority rule, federalism solves the problem of faction and eliminates the need for those prerequisites of successful republics, homogeneous population and small territory. The argument is interesting but provides more of a rationalization than an explanation for the creation of the federal system. Richards assumes that the creation of federalism is a rational act of will with predictable consequences. The actual constitutional resolution of the relation between the states and the federal government appears to have been more complex and ambiguous than this analysis suggests. The compromise on representation could have been an act of historical necessity or even an accident. The states existed, and there had to be some concessions to them to secure ratification of the new Constitution. Then, a costly Civil war resolved the conflict between different critical interpretations of the nature of the Union.

Richards's analysis of the adoption of the Civil War Amendments has similar difficulties:

> When the Fourteenth Amendment extended basic rights against state action, it did not innovate new rights but rather took standing guarantees of the 1791 Bill of Rights and expanded their application from the federal government to the states; one of the great lacunae (as Madison clearly saw) in the Constitution's protection of basic rights was thus filled. . . . These amendments were as much interpretative as innovative and reflect precisely the kind of critical interpretive arguments of principle that the founders used and anticipated that their posterity would continue to use. (p. 155).

This analysis is not likely to quell the objections of opponents of the incorporation theory. There is also no explanation for why the Court failed to apply this standard immediately after passage of the amendment, instead waiting until *Gitlow* v. *New York* (1925) to begin the process.

Richards's interpretation of historically continuous common principles legitimizes strong protections for the inalienable rights of conscience and limitations of political power. The theory also offers strong support for constitutional status for unenumerated rights (such as privacy) and government action to end discrimination. In the effort to develop a definite and persuasive theory of constitutional authority, Richards provides a provocative and interesting journey through the ideas and theories of the framers, as well as later periods. The outcome of his research is satisfying to all who want the first priority of U.S. Constitutionalism to be the guarantee of inalienable human rights. Unfortunately the analysis tends to be tendentious, and the "facts" sometimes seem forced to fit the theory. The claims of alternate interpretations continue to have force because achievement of an authoritative constitutional interpretation depends on a consensus that currently is not in evidence.

ROZANN ROTHMAN

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The Unchanging American Voter. By Eric R. A. N. Smith. Berkeley: University of California Press, 1989. 266p. $40.00 cloth, $13.95 paper.

This book provides powerful new ammunition for the battle over the image of average U.S. voters. Are they reasonably knowledgeable and improving or political ignoramuses who show very little improvement over the years? Eric R. A. N. Smith affirms the latter, reaffirming the verdict reached by Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes in *The American Voter* (1960).

This conclusion runs counter to the arguments presented by Norman H. Nie, Sidney

Verba, and John R. Petrocik in *The Changing American Voter* (1976) to support their contention that voters have become more politically sophisticated since 1960. Nie, Verba, and Petrocik, as well as many other political scientists claim that a politically volatile environment stimulated voters during the 1960s to learn more about politics and make more informed voting decisions. The two sets of evidence that were the mainstay of the argument were changes in the level of conceptualization index and changes in various indexes of attitude consistency during the period following the 1960 elections.

Dissecting each piece of evidence about the growth of voter sophistication under the microscope and scrutinizing it from various methodological perspectives, Smith concludes that the evidence is methodologically flawed and cannot sustain the argument. Specifically, the key indexes are time-bound and cannot be compared across time. Changes in question wording further confuse the issues. A check of other measures of political sophistication also fails to provide support for the change hypothesis. Hence, Smith concludes, changing political climates have not transformed the electorate. It remains composed of bored and ill-informed spectators of the political scene.

Methodological arguments ordinarily make dull reading even when the research question is exciting. Smith does a masterful job in keeping the writing crisp, the explanations clear, and the entire story interesting. The book is paced almost like a detective story, the author analyzing each piece of evidence and then fitting it into the gradually unfolding resolution of the puzzle. Smith even designed his own model of political sophistication to shed light on the causes of sophistication so that he could check whether these causes might explain growing sophistication of the electorate. The results sustain his initial conclusion and give little hope of imminent major changes in the electorate's qualifications for thoughtful voting.

Given the fact that election research is an area in which experts abound, the author's arguments will undoubtedly be challenged, as they ought to be. Although they are very carefully reasoned and supported by data and a careful analysis of the relevant literature, they are not airtight. For instance, one may wish to question whether the criteria by which the electorate's political sophistication is judged are appropriate in light of current knowledge about how people tend to process information and make decisions. One may even question whether these criteria are appropriate in light of the complexity of the political issues facing the nation and the nonideological character of election campaigns. Or, more fundamentally, one may ask whether the National Election Study data are sufficiently sound to support any conclusions, positive or negative, about U.S. voters, given the serious validity issues raised by Smith.

The Unchanging American Voter is bound to generate lively scholarly debate. In fact, provoking renewed debate on this important topic is the book's major contribution, along with the excellent arguments that it contributes to that debate. As Smith points out, current scholarly wisdom about U.S. voters rests on many unexamined assumptions that need to be reexamined before continuing the multitude of election research projects that are based on them. Even more importantly, the current debate about the need to restructure election campaigns may have to be recast. If Smith is right that voters are unchanged and unchanging, more sophisticated campaigns may be the wrong remedy for a misdiagnosed political malady.

DORIS A. GRABER

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Budget Reform Politics: The Design of the Appropriations Process in the House of Representatives 1865–1921. By Charles Stewart III. New York: Cambridge University Press, 1989. 254p. $39.50.

The Politics of Public Budgeting: Getting and Spending, Borrowing and Balancing. By Irene S. Rubin. Chatham, NJ: Chatham House. 248p. $16.95 paper.

Much of the literature on budgetary politics focuses on the federal government in the decades since World War II. But budgetary politics did not start on V-J Day and does not stop at the Beltway. A full grasp of the subject requires an historical perspective as well as a knowledge of how money is raised and spent at all levels of government. *Budget Reform Politics* helps meet the first need, and *The*

Politics of Public Budgeting addresses the second.

By today's standards, the federal government spent little during the years between Lincoln and Harding. But in the House of Representatives, says Stewart in *Budget Reform Politics,* "what little there was, was fought over fiercely" (p. 77). Stewart highlights a timeless feature of legislative life: the key battles are often about the structures of policy making, instead of policy itself. The era's lawmakers knew that what one Congress gives, the next can take away. Thus, they sought structural changes that could hamper future Congresses from veering from the course they had set.

Stewart observes that spending conflicts were fueled by election-minded politicians vying for pork. By the mid-1880s, rank-and-file pressures had led to a decentralized appropriations process that fostered spending increases. Stewart, however, does not stop there. He explains that the election-seeking behavior took place in an ongoing institution with committee members and party leaders whose goals went beyond hometown vote tallies. Moreover, House members had to contend with presidents who represented different political interests. All the while, the country was undergoing social and economic changes that remade the political landscape. Blending quantitative analysis and a close reading of the documentary record, Stewart shows how the interplay of internal and external forces led the House back to a centralized appropriations structure by the early 1920s.

As Stewart says, those "simpler days were not so simple after all" (p. 4). His rich analysis can supply a great deal of background to scholars of contemporary politics. Several times during the period under study, the country had a Republican president and a Democratic House—lately the norm. Stewart tells how House Democrats sought leverage over GOP presidents through rules changes that allowed substantive legislation on appropriation bills. In this way, he suggests the historical roots of recent issues such as the Boland Amendment controversy, in which House Democrats again used procedure to fight a president of the opposite party.

With Irene S. Rubin's *Politics of Public Budgeting,* we move from the Gilded Age to the present. It takes a panoramic view of fiscal politics, from city hall to statehouse to Capitol Hill. With a number of brief and illuminating case studies, it examines five distinct clusters of decision making: revenues, process, expenditures, balance, and implementation. The book explains budgeting in clear and practical terms, avoiding the jargon of political scientists and professional budget analysts. Although it points out similarities among the levels of government, it also notes the differences.

The book covers a vast territory in a readable space. Perhaps inevitably, it slights certain topics. For instance, it tells how officials can pass and implement a tax increase, but gives scant guidance about gathering support for spending cuts. A case study of, say, military base-closing legislation would have been helpful on this point.

Other topics merit more explanation than the book supplies. It says that social security taxes "are levied on individuals' paychecks and put into a trust fund that can be spent on nothing else" (p. 36). In fact, the trust fund is a bookkeeping entity. Social security contributions are commingled with other receipts and spent on many things besides social security benefits. In 1990 Senator Daniel Patrick Moynihan sparked a major debate when he referred to this aspect of the system as "thievery." Student readers could better understand such controversies if the book had given a fuller account of social security financing.

Nevertheless, the book's shortcomings are minor compared with its virtues. It will probably become a standard text in public policy courses, and future editions can fix any problems in the first.

The Politics of Public Budgeting concludes that when political figures seek more power, they often focus on the budget process as a way to get it—an idea that also guides *Budget Reform Politics.* While procedural changes affect the overall level of spending, both books find that such changes are not all-important. The federal budget did increase after the House decentralized its appropriations process in 1885; however, Stewart demonstrates that this growth also stemmed from a robust economy. Conversely, Rubin shows that there is more to budget restraint than the enactment of spending limits.

As for course adoption, the two books aim at different audiences. *Budget Reform Politics,* while written in sharp and largely nontechnical prose, is best suited for graduate students. *The Politics of Public Budgeting* would work well in a wide variety of graduate and upper-level undergraduate courses. Either makes a valuable reference for political scientists.

A comment by Rubin sums up both works: "Battles over budgeting processes, which might otherwise seem dry, technical, internal matters, turn out to be lively battles" (p. 238).

JOHN J. PITNEY, JR.

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Regime Politics: Governing Atlanta, 1946–1988. By Clarence N. Stone. Lawrence: University Press of Kansas, 1989. 314p. $30.00 cloth, $14.95 paper.

After more than three decades of studying the politics of Atlanta, one might reasonably expect community power researchers to have reached a point of diminishing returns. Nothing, however, could be further from the truth. Adolph Reed, Jr. recently built on an Atlanta case study to advance an important critique of neoprogressivism in theorizing about local development policy in *The Politics of Urban Development* (1987). One of the editors of that volume, Clarence N. Stone, now returns to the study of Atlanta in *Regime Politics;* and the results are important both for the study of Atlanta and for the study of urban politics generally.

Stone's book is important on three fronts. In the space of a single volume, he (1) extends our knowledge of four decades of Atlanta city politics; (2) extends the regime model of Steven Elkin (*City and Regime in the American Republic* [1987]) into an innovative and productive *urban regime* model that carefully mixes theoretical and empirical elements; and (3) extends the on-again-off-again community power debate once again by offering a "social production model" with which to analyze the role of business elites in local or urban regimes.

Stone tracks Atlanta's governing coalition of black middle-class leaders and white business interests across four decades, illustrating how projects that promoted development and provided opportunities for minority businesses have held sway, despite pressures from neighborhoods, the arts community, and preservationists. Both Maynard Jackson and Andrew Young, Stone argues, possessed the support to gain office but lacked the support to govern a city characterized by weak governmental authority and decentralization. Both mayors required, in the end, not the control, but the influence, of the downtown business community to help govern. Stone illustrates this dependency on business repeatedly as he charts the rise of the protest movement in the 1960s, the neighborhood movement in the 1970s, and, later, the electoral coalition of Andrew Young, all of which—in the context of a weak and decentralized governmental structure—eventually needed the help of the business community to govern.

This raises Stone's second point, namely, that properly understood (i.e., in light of the arguments of Charles Tilly's *Big Structures, Large Processes, Huge Comparisons* [1984]), the object of business elites is not to "control" so much as to influence, thus *empowering. Empowering* in this context should be understood as

> "to engage actively in formulating a policy agenda and in structuring a set of arrangements through which that agenda could be advanced. In doing so the business elite has relied on public authority and public revenue. The regime is thus empowering for the business elite: in partnership with local government, it can accomplish much more than it can alone" (p. 233).

Whether this view of business elites is true and whether (in its incarnation as an explanation of the behavior of local business elites) it improves on the classic explanation of "privatized" policy making by key interest groups and the drive to create "small constituencies" with the power to use public monies and authority for their own ends (Grant McConnell, *Private Power and American Democracy* [1966]) is debatable. However, precisely such arguments have revitalized the community power debate of late. Stone has done much to enliven this debate, and this book is yet another major contribution.

The key theoretical contribution of the book, and what may emerge in time as its most important contribution to the study of urban politics, is the development by Stone of an urban regime model. Again and again in the course of the book, Stone displays a deft ability to weave empirical examples from postwar Atlanta into a theoretical tapestry populated by vertical elites, horizontal (and, frequently, informal) coalitions, and classic U.S. tensions between popular elective control of the formal machinery of local government and the natural perquisites of private owners of key business enterprises. As Chester Barnard (*The Functions of the Executive*, 1938) has noted of informal organizations in general, understanding the informal organization is at least as important as understanding the workings of the formal organization—indeed, perhaps more so. Clarence Stone has taken on this task for the urban regime. It is a discussion likely to enliven the debates of urban politics scholars, as well as undergraduate and graduate courses for sometime to come.

ROBERT J. WASTE

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Intergovernmental Relations in the American State: The Johnson Presidency. By David M. Welborn and Jesse Burkhead. Austin: University of Texas Press, 1989. 328p. $40.00.

Intergovernmental Relations, by David M. Welborn and Jesse Burkhead, is a welcome addition to the scholarly literature on the Johnson administration. Part of an ambitious project on the administrative history of the Johnson presidency, this work focuses attention specifically on the administration of innovative social programs, and their subsequent impact on U.S. intergovernmental structure. The book is useful as both a historical examination of a rapidly growing administrative state and a study of the decline of the modern presidency. This is especially true considering the publicity given to the second installment of Robert Caro's hypercritical biography (*Means of Ascent*), which has made the life and presidency of Lyndon Johnson subject to increasing examination and historical reassessment.

Though its wealth of information makes this book valuable in several fields, it is ultimately about the erosion of U.S. federalism. However, the authors tend to distance themselves from federal theory throughout most of their discussion. Noting correctly that "the American administrative state had come to follow no neat or tidy path" (p. 6), Welborn and Burkhead briefly identify the major changes in federal-state-local relationships since the Constitutional Convention. Though each chapter has a conclusion that neatly summarizes major arguments, there is not, it seems to me, enough effort made to tie together the vast array of information into a coherent theory. This is by no means an easy task, and many have balked at it.

Welborn and Burkhead superimpose their findings onto existing constructs of federalism but give little credence to the continued relevance of federal-state *conflict*. Dismissing federalism (as envisioned by James Madison and the federalists) as an "artifact of . . . political philosophy" (p. 238), the authors' work becomes a description of the further nationalization of U.S. politics, with the states playing a central but subordinate role. The authors betray their biases by ignoring calls for restraint on national authority. Aaron Wildavsky has written that "uniformity is antithetical to federalism" ("Federalism Means Inequality," *Society* 22:5, p. 7) and Thomas Dye has recently called for a return to the federalism's "original intent." Federaism, then, is not an artifact of political *philosophy* but of contemporary U.S. politics.

Welborn and Burkhead are at their best when discussing the major War on Poverty programs—the Economic Opportunity Act of 1964 (OA) and the Model Cities program. These programs simultaneously altered certain intergovernmental power arrangements and reinforced others, creating manifold administrative conflicts. The authors chronicle the War from its intellectual roots (it was begotten, they assert, in the mind of John Kennedy and several Democratic members of congress) to its current salience. The difficulties involved in fiscal and programmatic expansion at the national level, which at the same time involves significant state and local responsibilities, are covered comprehensively. These sections serve as valuable studies of an unwieldy bureaucracy. The authors have examined other major initiatives of this reformist administration, addressing revenue-sharing problems, safe streets, health care issues, and specific examples of resistance from state and local leaders. The book clearly delineates the programmatic, fiscal, and administrative dimensions of the great society.

Richard Rose, in *The Postmodern President* (1988), has convincingly argued that, with the loss of U.S. global hegemony, it is incumbent on the president to recognize the limits of U.S. power and influence abroad while realistically assessing the ability of the economy to support new domestic initiatives. *Intergovernmental Relations* is an informative look at one of the last truly "modern" presidents. Through judicious use of data and tables (there are only seven throughout, all happily relevant) Welborn and Burkhead demonstrate the president's ceaseless optimism that "creative federalism" would overcome the difficulties of an overextended government.

Intergovernmental Relations has the advantages and drawbacks of a scholarly monograph. The research is exhaustive and does justice to the breadth of the topic, which is no small task. However, this breadth and commitment to detail often makes for bromidic reading despite the convenience that jargon is usually translated. For faculty, graduate students, and serious undergraduates this is appropriate, though undergraduates and casual students may find Welborn and Burkhead's verbiage formidable and their exacting accounts tedious. The book is meticulously indexed (there are two) and well edited although without a bibliography. Thus, though *Intergovernmental Relations* tells a fascinating historical tale, it is unlikely to figure in the current debate over LBJ. This is unfortunate, since it goes far to cast President Johnson's intentions into a mostly favorable light and is a good counterweight to Caro's popular history. But this volume will be indispensable to scholars who wish fully to understand an important and complex time in the development of intergovernmental relations and the presidency. Welborn and Burkhead should be commended for their definitive work.

DAVID QUAST

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COMPARATIVE POLITICS

The Patriarchal Paradox: Women Politicians in Turkey. By Yasim Arat. Cranbury, NJ: Farleigh Dickinson University Press, 1989. 162p. $26.50.

Yasim Arat's study is set against a bleak background. Since the establishment of the Turkish Republic, legal rights for women arose as part of a grand design for Westernization. Men rather than women insisted on recognizing women's rights; therefore, no women's movement was formed to define gender issues or to raise women's consciousness. The new civil laws, though an improvement in the legal status of women, had inherent male bias (p. 33). Despite increases in female literacy, still half of the women (as opposed to a quarter of the men) are illiterate. This makes Turkey's female illiteracy rate higher than in Jordan and Kuwait (pp. 34–35). Women make up 36% of the labor force; however, more than 90% of women are in the agricultural sector (p. 46). Since women's suffrage in 1935, female parliamentarians never exceeded 4.5% (1935–39) of the total membership; and in fact, they steadily declined to reach levels less than 1 percent in 1977–80 (pp. 52–53).

Politics and officeholding by women are the central themes of this work. The focus is female members of parliament (MPs), with partial attention to women in municipality councils (MCs) from Istanbul and Ankara. The bulk of the study is based on nonrandom sample interviews with 16 female MPs, 10 male MPs, and 12 MCs. The limitations and restrictions of the sample are acknowledged. The questionnaire addresses their background, education, occupation, social life, values, and attitudes.

The findings, carefully discussed, are revealing but not surprising. The female politicians assume responsibilities of a traditional housewife (p. 79). Their politicization process is sharply different from men; while men are politicized in response to a major political event, women become politically involved through male relatives (especially husbands or fathers) and party recruitment (p. 85). Female politicians do not view their election to office as a career but simply a promotion in the party ranks.

Male deputies are highly condescending toward women and emphasize that politics is not for women. Women show inconsistency; they claim that they are respected by their male colleagues and are warmly accepted by the electorate, but some blame men for small numbers of women in politics. Here, differences appear in the responses between female MPs and MCs. MPs blame the women more frequently than the MCs for their absence in politics (p. 111). The study does not explore the possible reasons. But in another place we are told that the female MCs were economically less powerful and affluent than the female MPs (p. 69). The possible significance of these *class* differences is never addressed. In fact, the study singles out the concept of *patriarchy* from varied theorists such as Simone de Beauvoir, Kate Millet, Shulamith Firestone, and Zillah Eisenstein but never discusses their differences. It merely asserts that they all agree that the structure of patriarchy endorses the power of male supremacy—an obvious point.

Discussing their differences and their views of each other (e.g., Beauvoir's view of Firestone) would have given depth and dynamism to the structure and conclusion of the study. Instead, the author asserts that all the above theorists have overlooked how women generate their own power (p. 19). This is debatable: gender, power, and class have been the eternal discourse in feminist theory. While the limited data reflect frightening urban-rural differentials, the author simplistically concludes that patriarchy is better than Marxism as an explanation (p. 121).

Throughout the work Islam is curiously omitted—except for a brief discussion of the religious background of the interviewees. Issues such as segregation, female MPs adherence to conservative dress codes, or problems with male-female intimacy—all in the Islamic cultural orbit—are raised; but any discussion of the impact of religion is evaded, including the rise of Islamic fundamentalism in the 1980s in Turkey, a highly sensitive topic to the Turkish government.

As the book draws to a close, a number of interrelated questions remain unexplored. What do the characteristics of female politicians tell us about their possible endorsement of legislation improving women's rights? Similarly, what do these women perceive to be gender problems in the Turkish polity? Can women who blame women help women? Can they ever understand the plight of the 90% female agriculturists? Would they ever question the patriarchal system, having lived at its mercy for so long?

This book is provocative not only for what it covers but also for what it does not. It is a useful study written cautiously in order not to offend. Freedom of expression is not available to all native scholars; we should commend those who try.

ELIZ SANASARIAN

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Beyond the Miracle of the Market: The Political Economy of Agrarian Development in Kenya. By Robert H. Bates. Cambridge: Cambridge University Press, 1989. 203p. $37.50.

Although those who have been caught in the vise of market forces might legitimately question whether markets are truly miraculous, this study of the political economy of Kenya's agrarian development deserves a wide audience. Theoretically rigorous, at times even elegant, this volume further confirms Robert Bates as one of the keenest observers of the complex relationship between politics and economics in the developing world. His formulations and findings are relevant not only in Africa but in other agrarian societies.

Bates has an ambitious agenda. Moving from the specific to the general, his first aim is to explain the sources of Kenya's exceptionalism. Unlike most other Africans, Kenyans generally value agriculture. Even urban residents usually intend to return to the land one day. The haves want to expand or embellish their holdings; have-nots wish to earn enough money to acquire land. A *shamba* (farm) is a near-universal goal. More to the point, at independence the Kenyan government was one of the few in Africa firmly committed to accumulation rather than redistribution.

In terms of theory, Bates hopes to expand the parameters of the political economy of development. In recent years neoclassical economists have seized the commanding heights of the terrain of development economics with their injunction to "get the prices right." But this school neither deals adequately with institutions nor seriously analyzes politics. Bates seeks to remedy this by demonstrating that "the study of institutions provides foundations not only for the study of markets but also for the study of political economy" (p. 4). The book thus attempts to integrate development economics and political science through an examination of how institutions such as lineages, parastatals, political parties, and electoral systems influence markets, thereby transforming and shaping economic interests.

Bates brings the state back in, but not as an abstraction. He examines the origins and roles of specific institutions, arguing they "play the same role in politics that markets play in economics: for rational actors, they create incentives that lead to characteristic choices. By creating a system of incentives, they help to account for the decisions taken by governments." These decisions—public policies—result from "a struggle among competing interests that takes place within a setting of political institutions rather than markets." Policy, therefore, is not the consequence "of some optimization process, which is subsequently distorted by private interests. Rather, policy is the product of the interested actions of private parties who bring their resources to bear upon politically ambitious politicians and the political process" (p. 5).

Bates begins with an examination of the agrarian roots of the Mau Mau Rebellion of the 1950s. Starting with a lineage-based model of Kikuyu society, he argues that the key variables in understanding the upheaval were the relative value of land in both the highlands and the reserves and the degree of access to land rights. When the colonialists moved to exclude squatters from the highlands after World War II, pressure was created in the reserves because the desire to retain high land values worked against the inclusion of the evicted squatters. Bates maintains that both the British settlers and the Kikuyu landholders in the reserves wanted the same thing: high land values. The result was a further restriction on access to land rights through a restriction of kinship and the creation of a landless class.

This analysis thus takes issue with those who have previously interpreted Mau Mau either in terms of nationalism (although he recognizes that Mau Mau brought independence to Kenya) or through a dependency perspective. There was no neat dichotomy between foreign capital and indigenous labor; both sides were divided and the struggle in the reserves resembled a civil war. Bates sees some merit in the moral economy framework but notes that the communities in this conflict were neither antimarket nor incorporative. Contrary to Kitching, Bates believes that the transformation of rural society leading to Mau Mau did not result in a petite bourgeoisie but in a rural gentry holding family-owned agricultural estates. Furthermore, the eventual political dominance of this class is a major variable explaining Kenya's exceptionalism.

Several things trouble me here. First, the author does not treat in detail Kitching's analysis of the state as a source of the off-farm income necessary to expand landholdings in the reserves. If the state was crucial in providing the venture capital necessary to acquire land, perhaps the dynamics of Kikuyu agrarian society were not quite as preponderant as Bates would suggest. Second, Bate's treatment of the rebellion would have been even more persuasive had he included an analysis of the urban revolt. While much of importance in Kenya can be traced to rural social structures, one needs to consider the urban dimension of the revolt, as well as the continuing salience and vitality of rural-urban linkages in both political and economic spheres.

The author then examines the alliances leading to the formation of two political parties, showing how these were rooted in the agricultural economy while reflecting an ethnic visage. The demise of the radical option in Kenyan politics, resulting, eventually, in the emergence of a one-party state, is also seen to be based in shifting patterns of agrarian political economy. Finally, contrary to the received wisdom, Bates demonstrates convincingly that food crises do not necessarily undermine governmental stability. The Moi regime was able to use crises in the late 1970s and mid-1980s to consolidate its power. What finally emerges is a consistently logical reinterpretation of the last 40 years of Kenyan politics.

Throughout, Bates masterfully expands on his theoretical arguments, demonstrating that it is necessary to anchor an analysis of class formation within an analysis of social structure and that one has to include an analysis of the broader polity within any study of agrarian politics. Institutions do make a crucial difference in the political economy of development, a lesson that those social scientists who are currently infatuated with the importance of prices would do well to ponder.

MICHAEL G. SCHATZBERG

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Conflict and Rhetoric in French Policymaking. By Frank R. Baumgartner. Pittsburgh: University of Pittsburgh Press, 1989. 287p. $34.95.

"Why do some issues become important societal debates, dominate the national media, and monopolize the attention of the nation's political leaders, whereas other issues are decided by small groups of experts?" (p. 3). Answering this question is the central purpose of Baumgartner's study. The empirical base for the book is a very narrow but extraordinarily intensive examination of policy making in France: thirty diverse cases of policy making handled by various divisions of the Ministry of National Education during 1983 and 1984. These cases are analyzed through 111 interviews with public and private actors, a content analysis of all coverage of education in *Le Monde* during the period in consideration, a mail survey of the major education interest groups, and selected documentary sources. This multifaceted assessment of representative cases drawn from a single issue area of a single country is designed to allow for the isolation of "those aspects of the policy and of its environment that cause variation in participation and public awareness" (p. 20).

Through an exhaustive quantitative analysis of his data and a qualitative assessment of selected cases, Baumgartner develops his central thesis. There is "only a moderate relationship between the objective scope of an issue and the level of participation that it stimulates" (p. 42). For more important than the scope of content of a policy is the degree of conflict it engenders: "Consensus leads to low levels of participation, and intense conflict usually but not always leads to high levels of participation. . . . Though intense conflict creates the incentives for policymakers to attempt to change the roster of participants, it does not guarantee their success in doing so" (p. 86). Conflict itself is often unrelated to the content of policies; for it may be generated by an environment featuring "categorical, institutional, and jurisdictional rivalries" (p. 94). Such independent variables as conflict, scope, and environment combine to determine the probability that a policy will generate relatively high or low levels of participation.

But "the single most important link in the chain" is the "strategic behavior of policymakers. . . . More than any other factor, their individual decisions and strategies determine the nature of the policy process" (p. 129). "Contractors" present "issues in the most narrow and technically complex way possible"; while "expanders"—those who seem to be losing the contest in the current specialized arena—"portray issues in the broadest and most political way they can" so as to bring allied political generalists (and even the public) into the process and change the balance of power in their favor (p. 93). "Depending on who wins the rhetorical battle" between expanders and contractors, "the policy community will be limited to a small number of experts, or it will be expanded to include large numbers of generalists" (p. 129).

Compelling though Baumgartner's argument seems in many respects, it is difficult to avoid the conclusion that he often exaggerates the degree of variance in participation primarily attributable to the rhetoric of policy makers. While they may have "a lot of room for maneuver" (p. 67), it is only in the rarest of conflictual contexts that they are able to achieve the sort of dramatic expansion described. Of his 30 cases, the three that came to "dominate the national media" were, by his own account, "the most conflictual" (p. 158). At the other extreme, in 10 cases where "conflict was minimal" expansion was not even attempted (p. 157). In the 17 other cases (involving moderate conflict) the success or failure of expansion efforts only slightly affected the "roster of participants." In the 10 cases said to feature unsuccessful efforts at expansion, the mean of Baumgartner's "index of participation" was $-.5$ (the mean of all 30 cases was .0, with a range from -2.75 to 9.14); and 6 cases attracted the attention of political generalists. In the 7 cases of successful expansion, the mean index was .3 and five cases drew in generalists (pp. 52, 157). Baumgartner concedes that "participation levels were not enormous" in this latter group, but insists that "even small changes can sometimes be important" (p. 158).

Though he may thus be faulted for overemphasizing the independent effect of "rhetoric," Baumgartner vividly illustrates how such variables as degree of conflict and environment combine with the strategic behavior of policy makers to determine the nature of participation and policy outcomes. His case studies provide not only the most systematic account to date of the dynamics of educational policy making in France but also a generally useful guide to the respective roles played by civil servants, interest groups, and political elites in the policy making process. Moreover, his analysis of the role of parliament as the "court of last appeal for potential expanders" in France (chap. 8) is quite enlightening, as is his explanation (chap. 9) of how political and structural differences between France and the United States have resulted in contraction of the debate over nuclear power in the former and expansion in the latter. Specialists in both French politics and comparative public policy will thus find this book to be a significant and provocative contribution to the literature.

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The Eagle and the Lion: The Tragedy of American-Iranian Relations. By James A. Bill. New Haven: Yale University Press, 1988. 520p. $30.00 cloth, $13.95 paper.

James Bill has written a most valuable and trenchant critique of U.S. foreign policy toward Iran in the Pahlavi and post-Pahlavi periods. The author has based his study on a great array of sources, including declassified U.S. government documents (plus those "declassified" by the student hostage takers at the U.S. embassy after their assault in November 1979); personal letters and memoranda; interviews with key policy makers; internal Iranian sources; and informal discussions with U.S. and Iranian public figures. Bill has accumulated a wealth of experience in his many years of travel to Iran and from his numerous studies of Iran's internal and external politics. The result is a work that is characterized by a high degree of authenticity and integrity.

Bill's thesis is that U.S. policy toward Iran since the 1940s has been characterized by deeply disturbing inconsistencies; willful intervention; fractured communications among the numerous official U.S. agencies both inside and outside Iran; endemic rivalries among members of U.S. civilian and military missions; cultural insensitivities and misperceptions; myopia concerning the alleged communist threat to Iran; and excessive reliance on contacts with Iran's governmental elite, especially the shah. Bill believes that these deficiencies were responsible not only for the United States' failure to anticipate and act to prevent an Iranian revolution but also for its inability to come to terms with the revolution after the fact.

Among the most compelling parts of this book are the discussion of the genesis of the U.S. commitment to the shah in the 1940s; the analysis of the interrelationships among the various U.S. and British oil companies; the impact of private sector figures (such as David Lillienthal of the Tennessee Valley Authority, David Rockefeller of Chase Manhattan Bank, and the newly retired but vigorously active Henry Kissinger) on U.S. policy; and Washington's catastrophic insistence that Iran approve a humiliating Status of Forces Agreement in 1964. With the exception of Truman, postwar U.S. presidents receive poor marks: Eisenhower because of his sanctioning of the CIA coup of 1953; Kennedy because his reform proposals were really intended to preserve the status quo; Johnson because he believed in the shah's use of force and admired his apparently stable rule, as well as his support of the U.S. war in Vietnam; Nixon because of his blank check of 1972 on arms transfers to Iran; Carter because he praised Iran's stability and later followed Brzezinski's hawkish advice to try to derail the revolution-in-progress by a military coup(!); and Reagan because of his demonizing of the Iranian regime and of course his involvement in the Iran–Contra scandal.

There are many lessons to be learned, and Bill provides a checklist of some 12 points for future consideration to avert the continuation of the tragedy in U.S.–Iranian relations. These range from a skeptical and questioning attitude toward any sacred cow assumptions in U.S. foreign policy making and prevention of private interests from overtaking public U.S. interests to quashing the urge to attribute all problems to communist activity, eschewing simplistic resort to force, fully training diplomats in all the skills needed to understand the societies to which they are posted, consulting more with nongovernmental sources for advice and information, moderating bureaucratic conflict, and instituting long-range planning.

It detracts nothing from the integrity of Bill's scholarship to note, however, that almost no one anticipated that a revolution would break out in Iran. It is also not patently obvious that the sort of subtle and orchestrated pattern of initiatives and responses preferred by the author to assist in the evolution of Iranian national development is in fact feasible. Given what we know about the enormously complicated problems of formulating and implementing foreign policy, it should be relatively clear that actions are often the outcome of unintended causes. This problem becomes all the greater when it is appreciated that revolutionary movements have a momentum and force of their own, a point made by Gary Sick in *All Fall Down* (1985). Even under nonrevolutionary circumstances, the foreign policy process can sometimes be such a bewildering series of fits and starts; assumptions and plans; bargaining, payoffs, and side-payments; initiatives and demarches; implementation and subversion—that it can defy efforts at careful rationalization. Finally, while great powers have historically found it relatively easy to roil the waters in their relations with small states, efforts at regulation of developments have been far less successful. The question must be starkly posed: What are the limits to great power ability to modulate trends in other lands?

Yet I take it to be James Bill's response that even if this ability is limited, it is possible for great powers to be less disruptive, less interventionist, less provocative, less insensitive, less domineering, less cavalier. Those who have studied U.S. relations with Middle Eastern countries such as Iran and in general with Third World countries deemed vital to U.S. security interests can only say *amen.*

SHAHROUGH AKHAVI

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Entrepreneurs and Politics in Twentieth Century Mexico. By Roderic A. Camp. New York: Oxford University Press, 1989. 306p. $35.00.

Law and Market Society in Mexico. By George M. Armstrong, Jr. New York: Praeger, 1989. 174p. $39.95.

No one seems to be able to come up with a term capturing the essence of the Mexican economic system. Massive state intervention rules out *capitalism* or *market economy; socialism* is hardly compatible with the enormous accumulation of personal wealth by a fortunate minority; and *planned economy* fails because plans are routinely ignored or aborted. Neither Armstrong nor Camp attempts to coin a satisfactory term; rather, they enrich our insights as to why Mexico's economic system seems so label-proof.

Armstrong employs a historical-legal approach in analyzing the origins of contemporary socioeconomic difficulties in Mexico. These problems, he believes, stem from enduring cultural traits and traditions that prevented Mexico from developing a materialistic society imbued with egoism and individual autonomy, which are prerequisites for a market economy. Communitarian paternalism, which was present in the initial blending of Indian and Spanish cultures, has endured; and in that context earlier practices of mortmain, entail, and debt peonage are directly connected with today's corporatist practices of the PRI, graft and corruption by public officials, and the "étatization" of the Mexican economy through state enterprises.

Camp is perhaps best known for his *Mexican Political Biographies, 1935–1980* (1982) and *Intellectuals and the State in Twentieth Century Mexico* (1985). In *Entrepreneurs and Politics* he continues to use personal interviews as the basic methodology. Essentially, the book is a report on empirical findings on the changing relationship between the entrepreneurial class and government. The analysis draws on an updated collection of political leaders (2,850 complete cases since 1884!), interviews with leading intellectuals, and his more recent interviews with prominent Mexican entrepreneurs. In addition, Camp compiled information on the interlocking associations of leading entrepreneurial groups and families who have ties with the two hundred top Mexican firms.

One of Camp's main theses is that the private sector sees itself as the dependent partner in a symbiotic, mentor-disciple relationship; but Camp argues that paradoxically, as the entrepreneurial class has matured and flourished under state tutelage, it is increasingly skeptical of the old-style, state-led model: "The relationship over the next decade will be tense and dynamic" (p. 252).

Neither volume lends support to a *dependista* explanation of Mexico's economic problems. Indeed, Armstrong explicitly and pointedly rejects a dependency position and highlights internal, atavistic attitudes, conventions, and organizational forms that have failed to be sufficiently receptive to external influences. Camp deals less directly with the issue; but it appears that both domestic entrepreneurs and politicos are not overly exercised over the presence of the U.S. multinational enterprises operating in Mexico. Both authors also agree that direct participation of the government in economic affairs is a basic and generally accepted ethos in Mexico (though Camp points out that there are powerful pockets of entrepreneurs, especially in Monterrey, that espouse a laissez-faire position).

Perhaps the most interesting contrast between the two works is that Armstrong's focuses on the perseverance of communitarian paternalism, while Camp devotes a great deal of space to change. Armstrong argues that imported ideas, including European liberalism and U.S. capitalism, never really had serious and lasting impacts on the legal system or on Mexican institutions in general. In his final two chapters he adduces both the explosive growth in the number of parastatal firms and Mexico's poor regard for intellectual rights as examples of modern manifestations of communitarian paternalism.

The conclusions of Camp's more lengthy volume are too numerous to recount here, but a selection should at least provide the flavor of his findings. Somewhat surprisingly, in view of the rapid expansion of the Mexican economy beginning in the late 1930s, family control of business still predominates over widely held private corporations. Camp is at his best when describing and analyzing evolutionary alterations occurring within and between the private and public sectors. For example, over the past few decades a higher proportion of entrepreneurs have urban origins, and they have become much more educated and less prone to recruit from groups with little prior exposure to the business world. These tendencies may be unwelcome news to those already alarmed by social and economic inequities in Mexico.

I do wish both authors had addressed the policy implications of their research more explicitly and fully. Yet these are two good books. I am especially keen on *Entrepreneurs and Politics*, which contains a superb collection of data on management directorships and stock ownership of major Mexican firms, revealing the rich tapestry of personal, family, and business associations among the elite corps of Mexican entrepreneurs.

As I write, Mexico is in the final stages of significant reductions in import licenses and tariffs, has recently liberalized regulations pertaining to direct foreign investment and technology transfer, and is attempting to sell over one hundred parastatal enterprises (including some large ones) to private investors. Is this merely a short-lived aberration from communitarian paternalism toward the use of more market forces, as Armstrong's analysis would imply? Does it presage greater harmony between the state and entrepreneurial class rather than the heightened tension that Camp predicted? I am confident that both authors will treat us to future analyses of these and other events in Mexico as they continue to extend and build on their firmly established research foundations.

DILMUS D. JAMES

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Pakistan: Transition from Military to Civilian Rule. By Golam W. Choudhury. London: Scorpion, 1988. 256p. $39.95.

One must sympathize with Choudhury on one point at least. According to an epilogue that he hastily appended to this book, President Zia-ul-Haq was killed in a plane crash less than a month after the manuscript went to the publishers. Such can be the fate of authors who write on current events, although one can only stand in awe of the efficiency of Scorpion Publishing if the publication process was so far advanced in less than a month that changes were no longer possible.

Choudhury's nominal purpose in writing this book is to describe and analyze the closely controlled transition that took place in Pakistan between 1983, when President Zia-ul-Haq realized that direct military rule could not go on indefinitely, and 1985, when the civilian government of Mohammed Khan Junejo was sworn into office. In the process, he delves selectively but usefully into the constitutional history of Pakistan (based largely on his previous writings) and argues the virtues of the Islamization of Pakistan's political system. Choudhury has been diligent in collecting official material on these topics and provides extensive excerpts but offers so little substantial critical commentary that one has the sense of working with Government of Pakistan press handouts. In these terms, this is a book of modest value.

Choudhury's pious and effusive handling of Zia's Islamization program is, perhaps, a matter best left to Pakistanis to judge. It would seem reasonable, however, to devote more than one bland paragraph to the highly controversial *hudood* laws imposing draconian Islamic punishments for crimes such as theft and adultery. Also, as far as Choudhury can tell, the status of women in Islamized Pakistan leaves nothing to be desired. Choudhury's discussion of Jinnah's attitude toward Islam and the state (pp. 85–89) will be questioned by specialists.

The transition from Zia-ul-Haq's open military rule to the more mixed form of government that he instituted was of course an intensely political process. Here Choudhury fails us badly. Although there is considerable good factual material, much of the discussion is purely structural, with little or no indication of the political dynamics that underlay it. Benazir Bhutto is dismissed as a "Western-educated young lady" unacceptable to devout Pakistanis as a leader (p. 56); the farcical referendum of 1984 is taken at face value and we are reassured that "no one can get away with announcing fictional election returns in Pakistan" (p. 64); in defense of Zia's emasculated *majlis-e-shoora* we are informed that the apparently sufficient role of a legislature in a modern democratic state "is to extract information and ventilate public grievances" (p. 55); the dismissal of the Junejo cabinet in May 1988, perhaps the most fateful step Zia ever took, is handled in less than a page's worth of text (pp. 180–81), most of it a reprint of the list of the allegations Zia made against Junejo.

The point of all of this is that Choudhury made very sure that nothing in his book could be read as a direct criticism of Zia. More seriously, Choudhury engages in shameless praise of Zia, turning his study at times into an embarrassing paean, better suited to official hagiographies of Kim Il-song—or perhaps a Mughal court biography. For example: "There is no gap between [Zia's] profession and actual practice of Islam. . . . Zia may be described as the first head of the state in Pakistan who not only preaches Islam, but also consciously practices it. His integrity, honesty, and uncorruptness are appreciated by the people of Pakistan" (p. 56). Or "Zia enjoyed wide popularity among the rural population because of his zeal for Islamization and rural development schemes" (p. 64).

Zia was a serious politician who deserved better than his critics grant him but also a more honest appraisal than Choudhury gives. Choudhury enjoyed good access to Zia (and made much of the fact) and showed him the manuscript before publication. This is hardly the stuff of which good political analysis is made; and, taken together with the sycophantic tone, it raises serious questions as to the real agenda of the book. Not only Zia is poorly served. The reader departs disappointed, and a work such as this adds no lustre to the corpus of one of our more distinguished observers of subcontinental affairs.

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The Manipulation of Consent: The State and Working-Class Consciousness in Brazil. By Youssef Cohen. Pittsburgh: University of Pittsburgh Press, 1989. 185p. $29.95 cloth, $12.95 paper.

Working-Class Mobilization and Political Control: Venezuela and Mexico. By Charles L. Davis. Lexington: University Press of Kentucky, 1989. 211p. $22.00.

In countries all over the world, workers often support governments that clearly act against their interests. In the highly inegalitarian societies of Latin America, such behavior is especially puzzling. Both of these books attempt to explain what Davis calls the frequent "political silence" of the working class. While they share some insights and a common methodology—surveys of urban workers—they provide answers that are fundamentally at odds.

Davis offers a "society-centered" explanation that draws on a survey of over a thousand Mexican and Venezuelan workers conducted between 1979 and 1981. He identifies the relatively small size of the formal sector proletariat in Latin America as a key obstacle to leftist parties that try to base themselves on the working class and middle class. This competitive disadvantage is exacerbated by the ability of dominant centrist and right-wing parties to dispense patronage and coopt working-class voters. In the end, workers fail to vote for leftist parties not because they are content with the way things are or because they don't understand or sympathize with the Left's message but because they simply don't believe leftist parties can win. Lack of meaningful electoral choice therefore best explains the tendency of workers to cast pragmatic if uninspired votes for the dominant parties.

Cohen, who conducted a survey of 617 urban workers in South and Southeastern Brazil in 1972-73, presents a "state-centered" explanation for working-class quiescence. He argues that Brazilian elites insulated workers within a protective but restrictive set of corporatist trade union structures in the 1930s and 1940s. At the same time, they successfully propagated an authoritarian ideology that justified the paternalistic practices of the state. This ideology endured over time, and in the early 1970s, at the height of repression by a military regime, Brazilian workers approved of the government because it fit their notion of what a "benevolent state" should be. Therefore elite manipulation and indoctrination—not lack of viable electoral choices—best explain working-class acceptance of the status quo.

Each of these arguments has weaknesses. Davis is unable to explain why leftist parties are sometimes able to surmount their competitive disadvantages and capture a sizable number of working-class votes. In Mexico, for example, Cardenas' Democratic Current received probably 30% of the vote in the 1988 presidential elections, with much of this support coming from workers. In discussing such a possibility (his book appears to have been written before the 1988 elections), Davis alludes to union leaders and their preference for "job unionism" over "political unionism" (pp. 141-46) and hints that economic crisis could erode this preference. But nowhere does he systematically develop this idea.

For his part, Cohen employs the concept of power in a rather one-dimensional way, to refer only to elite power over workers. The possibility that workers' organizations can exert power over elites is ignored. Thus, the reforms of the Estado Novo period (1937-45) in Brazil are described as concessions from above (pp. 86-93) rather than as compromises reflecting, in part, the bargaining strength of certain sections of the working class. Bargaining strength can enable workers to make a conscious, pragmatic choice for short-term sectoral gains at the expense of broad political goals, a strategy that Cohen, unlike Davis, does not acknowledge.

Cohen also has a curiously static view of working-class consciousness. He claims that Brazilian workers were highly receptive to elite ideas during the Estado Novo. However, once indoctrinated, workers maintained and transmitted these same beliefs for 30 years, remaining relatively unaffected by new currents in elite ideology. Cohen does not explain why this should be. Cohen also could have provided more context for his survey, in which 70% of the respondents said that workers should not be allowed to strike even if they had good reason (p. 40). These responses were obtained at a time when leftist parties had been expunged from the political scene and when a large expansion in industrial jobs had taken place—a significant and unique combination of circumstances.

Cohen asserts that state controls over trade unions serve to keep workers "ignorant, dependent, and servile" (p. 53) without specifying how this is done. His view is contradicted by Davis, who finds no relation between corporatist controls and autonomous worker attitudes and behavior in his study (p. 107). Similarly, Tamara Lothian has argued that corporatist labor laws and institutions are not inherently more repressive than those in the U.S.-style "contractualist" systems ("The Political Consequences of Labor Law Regimes," *Cardozo Law Review*, Summer 1986). It would have been interesting if Cohen had addressed some of these counterclaims. Interestingly, he provides evidence for them when he shows in chapter 7 how São Paulo metalworkers won significant concessions from employers and the state in 1978 despite the fact that the corporatist trade union structures remained intact.

In my view, Davis's book, while theoretically less ambitious and less well written than Cohen's, makes the stronger argument. His workers are not cyphers but people who live in towns and work in specific industries, which he describes. He categorizes respondents' answers according to whether the individual was in a union or not, in a strategic or nonstrategic industry, and so on, making careful conclusions and interesting comparisons.

Cohen's workers, on the other hand, are an undifferentiated and anonymous mass. (In this respect they are the opposite of the divided elites so vividly described in Peter McDonough's *Power and Ideology in Brazil* [1981].) Cohen makes sweeping statements about the beliefs of "the lower classes" and "workers" in Brazil that are provocative but ultimately mystifying. He edges towards the "working-class authoritarianism" thesis that he criticizes (p. 4) when he says that workers' families (presumably, all of them) foster "little autonomous, self-directed behavior" (p. 72), "limited cognitive and linguistic skills" (p. 80), and a belief that "what they get is what they deserve" (p. 70). Such overgeneralizations mar the book and obscure the reality that Latin American workers are self-conscious subjects rather than mere objects in society and can ultimately define the limits of the elites who claim to lead them.

In summary, each of these books stakes out a corner in an ongoing debate about working-class political behavior. Neither author uses new theoretical insights into how working-class experience shapes values and actions; but both serve up valuable empirical evidence in a field too often lacking in data.

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Japan's Administrative Elite. By B. C. Koh Berkeley: University of California Press, 1989. 312p. $38.50.

This is a study of the "life cycle of the [Japanese] administrative elite" (p. xii) that sets out to explore the major attributes of higher civil servants in contemporary Japan and compare them with both their pre–World War predecessors and their contemporary counterparts in other advanced industrial nations. Koh, best known for his previous work on North Korean politics and foreign policy, devotes the first three chapters of the book to short but informative discussions of the relevant secondary literature, the prewar Japanese bureaucracy, and the Occupation-sponsored civil service reform in postwar Japan. He then presents detailed descriptions of five specific aspects of the present-day Japanese national civil service in so many chapters: recruitment, promotion, socialization, modes of interaction, and rewards. These are followed by a brief summary and conclusion.

The book builds on, updates, and by and large confirms main conclusions of previous works on the subject, especially Akira Kubota's *Higher Civil Servants in Postwar Japan* (1969) and Robert M. Spaulding, Jr.'s *Imperial Japan's Higher Civil Service Examinations* (1967). Like Kubota and Spaulding, Koh finds the senior ranks of Japan's central government bureaucracy still dominated by a meritocratic elite that recruits its members predominantly from among law graduates of a few select universities, promotes them mainly on the basis of age and seniority, favors men over women and generalists over specialists, makes decisions by consensus, and tolerates and suffers from "sectionalism."

This is by far the most detailed, comprehensive, and systematic descriptive study of the subject available in either English or Japanese. It brings together and sorts out an enormous amount of interesting and useful information about the structure and behavior of the Japanese central civil service from widely scattered primary and secondary sources. This includes a great deal of information normally inaccessible to the general reader on, for example, the rules and patterns of promotion (chap. 5), midcareer, off-the-job training programs (chap. 6), and the pay system and incidences of corruption among senior civil servants (chap.

7). Moreover, much of the information is summarized and presented in a series of highly readable and eminently usable statistical tables. The book also offers the most detailed institutional descriptions of all of Japan's central ministries except the Ministry of International Trade and Industry, which is the subject of Chalmers Johnson's much cited *MITI and the Japanese Miracle* (1982).

The book, however, does have some problems. Relying as heavily on vernacular sources as he does, Koh takes chances, perhaps unavoidably, with the next-to-impossible task of correctly and consistently translating and transliterating difficult Japanese words and idioms. Considering the number and kinds of sources used, he actually acquits himself quite well on this score. The only slip of any real significance occurs, ironically, in the rendering of a common cliché, *tōkō seitei,* translated as "the ascendancy of party and the decline of *politics*" (p. 212, my emphasis), rather than *government,* as it should be. Far more numerous (but far less important) are missing, and occasionally misplaced, macron signs over Japanese long vowels; these occur in more than 30 places, overwhelmingly in footnotes. These may be annoying to some readers but do not detract from the overall scholarly quality and value of the work.

Koh's treatment of the subject suffers more seriously from his inability or unwillingness to subject the mountain of descriptive data to a critical and sustained analysis and interpretation. For example, he mentions the educational and occupational backgrounds of National Personnel Authority (NPA) commissioners (p. 69) without discussing the significance of the information in terms of, for example, the commissioners' work style or the NPA's institutional role. Elsewhere (pp. 74–76), he lists the fields of study covered in the Japanese civil service examinations without discussing the implications of the selection of the particular fields for, say, the behavior and performance of Japanese civil servants. In still another place (pp. 156–57), he notes that "individualistic" attitudes among Japanese youth declined between 1977 and 1983, without discussing either the causes or the consequences of the observed change.

The same lack of analytical depth and rigor also weakens the "comparison" Koh attempts of Japanese higher civil servants with their counterparts in the four other advanced industrial countries, namely, the United States, Great Britain, West Germany, and France. Each of the five core chapters ends with a brief recitation of comparable institutions and practices in each of these four countries. The exercise involves, however, merely parallel descriptions (based largely on secondary Japanese sources) of such institutions and practices, with virtually no serious comparative analysis to speak of.

Koh's book is thus a gold mine of detailed and reliable descriptive data about its subject that would easily lend itself to much fruitful secondary analysis either by its author or by others. As such a key source book, it is a rare and most welcome contribution to the comparative politics literature.

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Political Opposition in Post-Confucian Society. By Peter R. Moody, Jr. New York: Praeger, 1988. 289p. $42.95.

This is an interesting, comprehensive survey of the political cultures of East Asia, though it may have difficulty finding an appropriate audience. It would certainly be suitable for a graduate course on East Asian political culture. Indeed, the first few chapters would make excellent background reading for any course on East Asian domestic politics. (Unfortunately, students will have to wrestle with the obsolete Wade-Giles romanization system, which is even applied to contemporary China. How many students will recognize Deng Liqun in Teng Li-ch'ün?) It is unlikely that the book will contain enough new insights to excite area specialists, and it is too specialized for general college courses on Asia. Social scientists specializing in such fields as human rights or oppositional politics who come to this book seeking comparative insights will find some here and there, interspersed among lengthy accounts of only marginally relevant material that will be already familiar to East Asianists.

Indeed, given its title, surprisingly little is said about oppositions. Should not a book on political opposition in these countries devote more space on the barely mentioned subject of political imprisonment? In all these respects, the South Korea chapter is the best; but one would still like to see more on Confucianism's justification (or lack of justification) of repression and the effect of political imprisonment on the political process. In Japan the peculiar multiseat district system is described in terms of its effect on the majority party, whereas its fragmenting and marginalizing effect on the opposition would be more relevant to the subject of the book. Attention to this problem might have spared us the dubious prediction that if the parliamentary majority of Japan's Liberal Democratic party falls drastically, Japan will "come to resemble . . . polarized pluralistic systems" (p. 71). I find this inconceivable unless the electoral system were changed so that each parliamentary district only had one representative, obliging candidates to build electoral majorities (rather than merely corner a small sector of the vote, as is presently the case).

Sometimes Moody's arguments are difficult to follow. At the beginning he states that these societies' problems "are political and cultural more than economic or social" (p. 2), but he undercuts the "cultural" aspect by saying (correctly, in my view) that nothing in Confucianism is hostile to democratic values or institutions (pp. 4, 250). That leaves us with the residual unenlightening proposition that the problems are political. During most of the book there seems to be a reluctance to address structural problems that would make this make sense, though in the conclusion the author finds it "useful to think of culture itself as a structure" (p. 250).

One has to be troubled by any discussion of how a given society goes about "dealing with political opposition" (p. 75). If Moody is correct that these societies are weak (meaning to the state), it cannot be the society that does the "dealing." Is it the culture? Well, a group that gains control over the state by force has no greater (and probably less) claim to represent the culture than do conscientious citizens who go to prison rather than embrace the regime. This is a theoretical problem that Moody never really addresses. We are told that "appeals to specifically western values come mainly from the opposition." But why is the demand for majority rule less indigenous than rulers' defense of minority rule? And how heuristic is it to think of these cultures as "post-Confucian"? Certainly, the Taiwanese, who had enjoyed rule of law under the Japanese, see Confucianism pretty much as they see anticommunism: ideological attempts to legitimize a regime that has had little else in the way of legitimacy.

Of the various country studies in the book, it is primarily with regard to Taiwan that the author can be faulted for lack of political balance. (I omit from this calculation his sympathies for the fomer South Vietnam regime.) During the 1970s and 1980s the Taiwanese opposition had much less opportunity to convey its message to the public than one would assume from the chapter on the subject. The author cannot find a motive for the various murders of opposition figures, uncritically echoes the authorities' disinformation about the 1979 Kaohsiung Incident, and fails to mention the central fact of Taiwan's elections: one-party control of the broadcast media.

Of the countries covered in the book, North Korea's opposition has probably fared the worst. Moody's coverage is quite inadequate, and the interested reader would do well to refer to Richard Kagan's *Human Rights in the Democratic People's Republic of Korea* (1988). Moody's chapter on communist Vietnam is also thin.

This book was published just before the political reversals in China (not to mention the subsequent surprises in South Korea, Taiwan, and Mongolia). Had it been published later, some sentences would certainly have been altered. One cannot say whether Tiananmen would have forced the author to make fundamental changes; but one would hope so, in view of the virtually unanimous approval that the citizens of Beijing gave the opponents of the regime (according to polls). In studying political culture, one now has to take oppositions in China (and probably all of East Asia) much more seriously than hitherto and the all-powerful states perhaps a bit less seriously.

I have doubtless placed excessive stress on points of disagreement. Indeed, this is a stimulating, well-informed volume; and no one who reads it will fail to profit thereby.

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Book Reviews: Comparative Politics

The Politics of Left-Wing Violence in Italy, 1969–85. By David Moss. New York: St. Martin's, 1989. 317p. $39.95.

Democracy and Disorder: Protest and Politics in Italy 1965–1975. By Sidney Tarrow. New York: Oxford University Press, 1989. 399p. $59.00.

The events described in both these excellent volumes must inevitably be seen against the background of recent European-wide political developments. In the middle of 1990 the prospects for working-class revolution waged on behalf of Marxist–Leninist principles do not appear particularly bright, to say the least. These days even the term *proletarian struggle* seems antiquated, more like the libretto for *Cavalleria rusticana* than a blueprint for the future. Yet not all that long ago, many Italians—young people especially—were enthralled by the possibilities of just such a revolution and used the vocabulary as if its very invocation could make their dreams come true. If for no other reason, these two books are important because they carefully chronicle and analyze what may later come to be regarded as the last significant attempt to make a Marxist revolution in Western Europe.

Democracy and Disorder, the wider-focused of the two volumes, should be considered first. Sidney Tarrow is concerned about the wave of mass protests that occurred throughout most of Italy over the decade 1965–75. In his view these protests were the outgrowths of the severe strains that changes in Italian society had brought to bear on the lives of millions of citizens. The consequences of these strains, along with the opportunity to express them in the political arena, was a "cycle of protest." Using data on the attributes of protest events, innovatively coded from the *Corriere della sera,* the author provides us with a theoretically informed account of the rise, spread, and decline of these occurrences. Much of the book is focused on the diffusion of protest activity from one group to another—students to workers to white collar employees, and so on—as the cycle escalated and what the author refers to as the "social movement sector" expanded. Attention is also paid to the repertoire of techniques the discontented used to convey their grievances to those in power.

When the repressive apparatus of the state, along with other factors, combined to make direct action more costly, the cycle of protest spiraled downward. The result of this process, the author tells us, was an intensification of competition among new movements and old organizations for the support of a dwindling pool of constituents. In turn, this situation ironically produced both an increase in the pace of institutionalization among the groups committed to protest and an increase in the level of small-scale political violence as leaders strove to keep momentum up and their hopes alive.

The elegance of this argument concerning the nature of a protest cycle cannot be conveyed fully by such remarks. But by treating the cycle almost as a ballet of discontent that began in the mid-1960s and ended in the early 1970s, the author underestimates the extent to which mass protest has been a continuing feature of Italian politics in the postwar era. For example, the book makes no reference to Ted Gurr's finding that Italy displayed the highest magnitude of civil strife of any European democracy in the 1961–65 period, that is, before the cycle of protest is conceived as having begun. Nor is mention made of 1977, the year in which there was a substantial outbreak of mass protest in Bologna, Rome, Turin, and several other cities—all this after the cycle of protest had apparently ended.

The Politics of Left-Wing Violence in Italy, 1969–85 offers the most thorough account of revolutionary terrorism in Italy currently available in English. Its author's theoretical perspective is guided by the concept of "translation." David Moss reasons that the acts of violence with which his book is concerned have no intrinsic political meaning. Rather, they must be made meaningful through a process of public translation. Perpetrators and victims, as well as self-interested bystanders, compete with one another to assign political significance to what might otherwise be regarded as unintelligible by the publics to whom most of these acts are addressed. Illustratively, the authorities refer to the violence as "terrorism," while the same deeds are defined as "armed struggle" by those committing them.

Working from this perspective, the author proceeds to explore the course of left-wing political violence committed by small groups in Italy over a period of more than 15 years. Little of any major importance is left out. The violent events are analyzed, as are the attributes of both the individuals and groups responsible for their occurrence. The reader is furnished with detailed accounts of the reactions of the state (government, parliament, judiciary), political parties, and labor unions to these *anni di piombo* (years of lead).

What does *Democracy and Disorder* have to do with *The Politics of Left-Wing Violence in Italy?* In other words, what did the cycle of mass protest have to do with the episode of revolutionary terrorism that followed in its wake? The authors disagree with one another on this question. The emphasis of *Democracy and Disorder* is on discontinuity. Terrorist violence came at the very end of the protest cycle and had little to do with the mass demonstrations and other activities that were its hallmark. On the other hand, for David Moss the relationship was far closer. For example, he notes the frequency with which such terrorist groups as Prima Linea (Front Line) drew their adherents from members of the *servizi d'ordine* (security forces) of the several New Left mass movements. And he also reports the difficulty many FIAT auto workers had in distinguishing the activities of the Red Brigades from those pursued by the extraparliamentary movements seeking to win their support. This dispute seems likely to endure for some time. In any case, both books should be read by all those interested in Italian politics over the last two decades.

LEONARD WEINBERG

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The Czechoslovak Economy 1948–1988: The Battle for Economic Reform. By Martin Myant. Soviet and East European Studies, no. 65. Cambridge: Cambridge University Press, 1989. 316p. $54.50.

The Origins of Backwardness in Eastern Europe: Economics and Politics from the Middle Ages until the Early Twentieth Century. Edited by Daniel Chirot. Berkeley: University of California Press, 1989. 260p. $35.00.

The Hungarian Model: Markets and Planning in a Socialist Economy. By Xavier Richet. Translated by J. C. Whitehouse. Soviet and East European Studies, no. 64. Cambridge: Cambridge University Press, 1989. 211p. $44.50.

The Crisis of Marxist Ideology in Eastern Europe: The Poverty of Utopia. By Vladimir Tismaneanu. London: Routledge, 1988. 232p. $35.00.

There are good reasons why the collapse of communist rule in Eastern Europe should attract fresh scholarly attention. The emerging regimes furnish both the rationale and information for a reconsideration of the past, while their consolidation—in whatever form—begs questions as fundamental as any in the postwar era. Apart from their respective limitations, these books discuss ineluctable questions for a serious examination of the region. Why is Eastern Europe politically distinctive? What considerations help explain the diversity and uniformity of socialism in the region, and what have been the obstacles to change?

Chirot's volume comprises seven uniformly good essays, whose organizing concept, nevertheless, amounts to little more than a reminder that Eastern Europe was backward because its productive forces lagged behind those of the West. Generally, the authors consider that the source of this condition lies in stagnant agrarian relations compounded by poorly developed internal markets and politically weak cities. Solid data in the essays of Jacek Kochanowicz on Poland and Peter Gunst on the region as a whole confirm this finding. Gunst finds Eastern Europe "bifurcated" (p. 72) from the sixteenth to eighteenth centuries. The economies of Poland and Hungary depended on autocratic manors and socage labor despite their commercial involvement with Western Europe, while the economies of Romania and Russia, based on rent and sharecropping, were even more inimical to development. Thus, it is difficult to sustain Immanuel Wallerstein's explanation of a pernicious dependency suffocating potential industrialization. Indeed, John Lampe, citing recent Bulgarian scholarship, finds that the Balkans ran an export surplus with Western Europe through the eighteenth century, while Ottoman terms of trade "improved" (p. 185). The revival of Ottoman-Hapsburg warfare subjected the Balkans to levels of disorder, disease, and migration that further reduced the already-low levels of internal trade and its population density.

A brief review cannot address the volume's abundant detail, such as the role of Jews in urban life and ethnic Germans in Bohemia. Bohemia's peasantry and commercial development are exceptional to the region. Gale Stokes imaginatively applies Barrington Moore's framework explaining nationalism and class relations to the region in order to account for the variety of political cultures. The Czech lands afforded valuable political experience through burgher-dominated governing boards and diets sharing power with Vienna's appointed governors. In Romania, an aristocracy more dependent on the monarchy than in Hungary helps explain the former's "weak tendency" (p. 233) toward interwar democracy and extensive land reform. The absence of landlords in Serbia and Bulgaria promoted strong peasant parties. Stoke's recognition of the particular role of individuals and external circumstances for each state is characteristic of the volume's cautious sensitivity to the region's abundant detail.

Unlike the scholarly tone set by Chirot's contributors, Tismaneanu's expresses an angry and brilliant indictment against "mythocratic" Stalinism (p. 51) as well as a patient account of revisionism and critical Marxism's failure to promote its reform. The author's acknowledged debt to Hannah Arendt, Lezek Kolakowski, and Czeslaw Milosz does not lessen the book's originality. Few writers have better conveyed the sense of "powerlessness, painful rootlessness and . . . total lack of perspective" (p. 173) experienced by East European intellectuals. The author also explains how prewar intellectual traditions and subsequent levels of repression have combined to create different expressions of dissent and opposition. In the case of Romania, extirpation of interwar culture and the weakness of a political Left had left a timid opposition, while the Czech Vaclav Havel and the Pole Adam Michnik persisted in the expression of truth against adversity. Tismaneanu's Manichaean world is tempered by his sympathetic treatment of such critical Marxists as George Lukacs. Ultimately, however, the death of Marxism comes to be founded on an "unshakable conviction" of the region's important thinkers that "all grandiose designs are ultimately instruments of political domination" (p. 175).

The collapse of communist rule in Eastern Europe lends credence to Tismaneanu's assertions, but his diagnosis of Soviet intentions is less convincing. Certainly, the defiance of Soviet domination has not proven to be "a quixotic, almost suicidal endeavor"; nor is it obvious that the party apparatus and *nomenklatura* remain Gorbachev's "power base" (p. 207). The author seems unable or unwilling to recognize that Gorbachev's reforms could critically undermine the political system that brought him to power.

Through his detailed account of the Czechoslovak economy and its abortive reforms, Myant provides a certain counterpart to Tismaneanu's record of Stalinism in intellectual life. While Chirot's contributors recognized the exceptional case of Bohemia, Myant notes how the imposition of central planning created in modern Czechoslovakia a condition of "backwardness relative to the most advanced countries . . . and to a widening gap [between them]" (p. 235). The problem rests less in the well-known dysfunctions of central planning than in a peculiar process of post-Stalinist bargaining over production targets. Growth became hostage to unrealistic investment and political stability to relatively predictable living standards. Sudden shortfalls in investment, swings in CMEA trade and energy prices create short-term crises against the background of declining competitiveness, commercial isolation, and an organizational morass. For example, a small passenger car conceived in 1955 becomes ready for marketing in 1988!

The anecdote is exceptional in a book that would deemphasize political influences were they not so pervasive to the subject. Myant chronicles the ineffectual attempts at reform and economic debate culminating in Alexander Dubcek's replacement of the "chronically indecisive" (p. 157) Anton Novotny as party secretary. The promise of authentic self-managing firms raised in 1968 is not repeated. Myant is to be commended for his masterful use of economic data, but his treatment neglects to mention the appalling environmental costs of the economic system and begs a major question. Which factors were responsible for the virtual absence of demands for change external to the system's core?

Likewise, Richet does not interpret Hungary's New Economic Mechanism (NEM) within the context of a distinct political culture, yet the NEM works through a National Planning Office whose "own logic" (p. 108) differs from that of the party apparatus. The planning process permits the operation of lobbies—"informal groupings attached to some major branch of the administration but not identical with it" (p. 111). As Tismaneanu emphasizes the 1956 revolution in shaping Hungary's innovative Marxism, so Richet considers it the "starting point of future [economic] reforms" (p. 8). Richet's work nicely complements Gati's *Hungary and the Soviet Bloc* (1986) and Revesz's *Perestroika in Eastern Europe* (1989) in offering a clear exposition of the massively regulated function of planning, prices, and profits that accompany Hungary's market socialism. Like Myant, Richet fails to mention environmental costs of the Hungarian "model"; and though available elsewhere, Richet could have better supported his description with performance data. The absence of attention to some measure of Hungary's competitive position in foreign trade is especially annoying.

Richet does help dispel the mistaken perception of a Hungarian economic synthesis. Instead, a hybrid that is "neither plan nor market" (p. 184) virtually monopolizes investment from the center, protects inefficient industry from competition, and preserves a "certain *de facto* aversion" (p. 108) toward the decentralized initiative it is intended to promote. Since the political changes that have taken place in Hungary and elsewhere in 1989, it remains moot whether plan and market could ever be reconciled.

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Inequality in Africa: Political Elites, Proletariat, Peasants, and the Poor. By E. Wayne Nafziger. New York: Cambridge University Press, 1988. 204p. $42.95 cloth, $12.95 paper.

Mechanization and Maize: Agriculture and the Politics of Technology Transfer in East Africa. By Constance G. Anthony. Columbia University Press, 1988. 178p. $25.00.

Wayne Nafziger and Constance Anthony have written two comparative studies of modest length dealing with two different aspects of African development. An economist, Nafziger is concerned with the multiple dimensions of inequality that result from the fiscal, price, and exchange rate policies of what he terms "the managerial bourgeoisie" and the impact of inequality on economic growth and political life. By contrast, Anthony explores the process of technology transfer for agricultural development.

Wayne Nafziger begins his analysis of inequality in Africa by considering the experience of the continent in its global context. After noting that economic growth is usually accompanied by inequality, Nafziger argues that the trade-off between growth and equity is not inevitable, that several developing countries which have experienced high rates of growth have had lower levels of inequality than most African states, and that states that have pursued socialist strategies of development (both African and non-African) have not always succeeded in promoting equity. Nafziger also makes the important distinction between alleviating absolute poverty and achieving equality by noting that the proportion of the population below the poverty line may fall as the overall level of inequality rises.

Nafziger then explores the multiple dimensions of inequality by presenting a wealth of statistical data from eight African countries. These include data on personal income, individual access to education and health, urban-rural disparities in the provision of social welfare services, prices paid to the producers of agricultural commodities, disparities between regions, and the impact of overvalued exchange rates. All have contributed to a record of declining or negative growth, and immiserization among those who are already poor. Nafziger then argues that African countries can reverse this record and achieve a measure of both growth and equity—but only if the state and the "managerial bourgeoisie" that staffs it draw back from policies that serve its narrow interests (e.g., low producer prices to farmers and overvalued exchange rates). He also notes that political repression in Africa is closely correlated with declining growth and rising inequality.

Despite the wealth of data presented by Nafziger, *Inequality in Africa* suffers from two limitations. First, Nafziger's argument is essentially the same as that made by Elliot Berg in his 1981 report for the World Bank. While Berg and his associates dwelt on the extent to which state participation in the economy retarded growth, he also argued that low producer prices for farmers and overvalued exchange rates exacerbated inequalities in personal income. Like Nafziger, Berg also argued that policies that promote growth in agriculture also reduce some aspects of inequality.

Second, although Nafziger's book traces the emergence of inequality from the colonial period to the present, little of the quantitative data presented is given in a time series format so that the reader can follow the extent to which inequality has become more pronounced over time or how the level of inequality is a function of other variables. There is a disjunction between Nafziger's narrative (which is historical in nature) and much of his data analysis (which is cross-sectional). Nafziger's citations of his sources of data are also woefully inadequate, at times nonexistent.

Notwithstanding these limitations, Nafziger has presented the obverse side of what is now the standard argument for structural adjustment reform in Africa. By focusing on inequality, Nafziger has written a useful companion to the Berg Report and its successors from the bank. Nafziger's book is a broad yet concise examination of an aspect of development largely neglected by non-Marxist scholars and one that is particularly good for advanced undergraduate and graduate students seeking a quick grasp of this important issue.

In her book on the relationship between technology transfer and politics, Constance Anthony explores a realm that has not been given extensive attention in the literature on African development but that is important to the extent that technology choice determines the winners and losers in the developmental process. Anthony's book considers two aspects of technology transfer for agricultural development: mechanization and the Green Revolution in respect to maize. She argues that the process of technology transfer, especially the choice of what specific technologies are transferred, is shaped by the interplay of three factors: (1) the international donor agencies and private corporations that import First World technology into Africa and sponsor research to refine such technology on the continent; (2) the political regime of the country into which the new technology is introduced; (3) the nature of the farming systems and ecology in the host country.

In comparing technology transfer in respect to the mechanization of agriculture with that of the Green Revolution, Anthony draws on the concept of an "international regime" (as articulated by John Ruggie) by arguing that successful transfers of technology are more likely to occur where an international regime has been established in respect to the technology being transferred than where it has not. Given this reasoning, the introduction of mechanized farming in Africa has partly failed because the providers of the technology (the UN Food and Agricultural Organization, NGOs and others) have been unable to agree on a common strategy and choice of technology. By contrast, the Green Revolution has experienced a significant measure of success because of an international regime—in this case a global network of research institutes on agriculture supported by the Consultative Group on International Agricultural Research (CGIAR), which includes all major bilateral and multilateral donors of foreign aid—while Anthony implies that the existence of an international regime is a necessary if not sufficient condition for technology transfer, the approximate weight that should be given to this variable is never specified.

The remainder of Anthony's presentation is less convincing, especially her discussion of the impact of the political regimes in Kenya and Tanzania, the two countries that serve as case studies for her analysis. According to Anthony, Kenya's capitalist-oriented regime distorted the process by which tractors where introduced into the African farming sector by diverting the flow of the technology to large-scale wheat farmers who were mainly Kikuyu tribesman and supporters of the government of Jomo Kenyatta. While it is true that the wheat farmers received most of the tractors, Anthony gives insufficient weight to the fact that most farmers in Kenya, including most Kikuyu farmers, are smallholders, for whom tractor technology is inappropriate. It is a rather simplistic understanding of Kenyan politics to explain the spread of tractor technology mainly in these terms, especially when the author is clearly aware of the ecological constraints on both tractor and Green Revolution technology in East Africa. To argue that tractor technology was perceived in the two countries as a tool for state building (p. 85), is also not consistent with the political structures employed by both regimes. In neither country was tractor technology ever a significant mechanism for establishing political authority vis-à-vis small farmers compared to clientelist machines (Kenya) or party institutions (Tanzania).

Anthony's discussion of the political underpinnings of the transfer of Green Revolution technology into East Africa is also questionable. To argue that the introduction of hybrid maize in Kenya was never intended for the smallholder farmers but that the technology slipped through the state's capacity to limit its adoption to largeholders misses the political logic of the Kenyatta era. While the Kenyatta regime certainly favored largeholders, much of its political base, especially in Central Province, came from small farmers. Indeed, the dramatic spread of smallholder tea and coffee production, assisted by the World Bank and the introduction of new technologies via the Kenya Tea Development Authority, indicate that where the regime could assist smallholders, it did.

While Anthony's triadic model of analysis is intriguing, her application is insufficiently nuanced in respect to the social base and agenda of the political regimes in Kenya and Tanzania. The book is also hopelessly dated, with virtually no consideration of events or the relevant literature after 1975.

JOEL D. BARKAN

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The Naxalites and their Ideology. By Rabindra Ray. Delhi: Oxford University Press, 1988. 251p. $19.95.

Most studies of the Naxalites, Ray argues, have obscured a central paradox, namely that a movement that proclaims commitment to agrarian revolution gives priority to urban terror. Rejecting the notion that the movement "degenerated" into "individual acts of terrorism," he identifies a terrorist perspective at the heart of Naxalite ideology. Rejecting the common assumption that the Naxalite movement resulted from certain socioeconomic conditions, Ray associates its genesis with the interaction between ideology and action.

At the heart of Ray's complex analysis, elaborated in chapter 1, is an exploration of the linkages between the Naxalites' "literate ideology" (the leadership's published statements and documents) and their "existential ideology" (the meaning of Naxalite discourse to the movement's participants). "Separated in meaning," he argues, "this literate ideology and existential ideology are nevertheless related to each other and transformed into each other, if by nothing else than the activity of the Naxalites" (p. 5).

Three subsequent chapters provide the disparate building blocks of Ray's analysis. Chapter 2 criticizes instrumental understandings of ideology, particularly Leninism, for emphasizing the inner affinity between particular ideas and social groups. Ray then describes the features of Bengali society (chap. 3) and political history (chap. 4), which condition the emergence of the Naxalite movement, merely hinting at the connections to his overall argument.

The four remaining chapters boldly set forth Ray's scathing indictment of the Naxalite movement. By analyzing the pronouncements of Charu Mazumdar, the movement's chief theoretician, Ray identifies glaring contradictions between the party's emphasis on discipline, openness, and inner party democracy and its highly anarchic, secretive, authoritarian style. Most strikingly, the movement was silent about its habitual use of terror.

Ray's explanation for the disjunction between the Naxalites' literate and existential ideology centers on Mazumdar's emphasis on strategy divorced from theory. An excellent example concerns Mazumdar's characterization of the Indian state and political economy as "semi-feudal, semi-colonial." Yet the only evidence the revolutionaries can muster for semicolonialism concerns the vestiges of British influence in the universities. In this paradigmatic instance, the privileged experiences of student cadres become the vantage point of the movement as a whole. At the same time, students experience guilt and ultimate self-hatred at their "petty bourgeois" backgrounds. Literate and existential ideology thus merge as the nihilist ethic of the revolutionary expands to engulf the entire society.

Ray's powerful critique of negation without affirmation, methods divorced from objectives, and politics reduced to technique has far-reaching implications for the study of social change. But how persuasive is his indictment of the Naxalite movement? Ironically, Ray's own theoretical and methodological premises lead him to reproduce certain tendencies that he stridently criticizes. Although he contends that the Naxalite movement can only be understood by considering the perspectives of participants, he never describes the ideology of rank-and-file members. Indeed, Ray, like the Naxalites, makes Charu Mazumdar into a demigod. By depicting the movement as a unified whole, Ray obscures the fact that many prominent Naxalite leaders denounced the same nihilist tendencies he criticizes.

Ray's principal source of information about the Naxalite movement is the overseas edition of *The Statesman*, which is part of the establishment press. Yet reserving the term *partisan* for Marxists, he fails to assess its biases critically and draws on an exceedingly narrow source of information. Moreover, Ray's literal reading of Naxalite documents ignores both their propaganda function and the polemic with parliamentary Communist parties in which they were engaged.

An even more serious problem concerns Ray's failure to periodize the Naxalite movement and to dismiss its earlier, less violent, agrarian phase. If, as Ray notes, Marxist scholars have overemphasized the socioeconomic causes of the movement, he errs in the opposite direction. By describing the Naxalbari uprising as an "originating myth," Ray becomes guilty of the very elitism he attributes to Naxalite leaders. For he renders insignificant the very substantial involvement of the rural poor, particularly tribals, in several regions, including Naxalbari (1967), Srikakulam (1968), Khammam and Warrangal in Andhra Pradesh (1969), and Gopiballavpur in West Bengal (1969). Indeed, one might argue that urban middle-class revolutionaries derived energy and inspiration from an earlier tradition of tribal radicalism that was prompted in turn by a nexus of material, cultural, and political influences. Recall the outbreak of peasant protest in far-flung regions of India in the late 1960s, prompting Home Minister Y. B. Chavan to speculate that the Green Revolution might not remain green.

By neglecting to consider the material underpinnings of political radicalism, Ray's analysis takes on the highly subjective quality that he associates with the Naxalism. That the Naxalite movement persists—and has even experienced a certain resurgence as it has learned from its past errors—suggests the need to avoid blanket condemnation and engage in more nuanced assessment of its evolution.

AMRITA BASU

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Ordinary People in Public Policy. By Richard Rose. London: Sage, 1989. 189p. $45.00 cloth, $17.95 paper.

At a time when the value of many government programs to the middle class is widely debated in the United States and in Great Britain, this book offers a thought-provoking overview of issues that merit further research and discussion. With the goal of bringing "ordinary people into the study of public policy by examining the extent and also the limits of the impact of public policy upon ordinary lives" (p. 2), Rose presents a stimulating examination of a number of facets of modern citizenship. Ordinary people are defined as typical, mainstream, middle class. They are not the rich, the powerful, nor the very well educated but rather typical citizens who are the beneficiaries of a variety of public policies. Rose writes: "Ordinary people have many roles: in the course of a single day an individual can be a spouse, a son or daughter, a producer, a consumer, a friend, and even a citizen. Non-political roles are far more numerous than political roles, and ordinary people spend far more time in non-political than in political activities. That is why the priorities of policymakers are not the same as the priorities of ordinary people in everyday life" (p. 175). This perspective is similar to that of the "social history" that is gaining prominence in other disciplines.

While reviewing citizens' patriotic attitudes toward their governments and factors influencing their voting, Rose's major interest is in what government does for these people. In a comparative context (mostly United States and Western Europe), Rose examines the private benefits of public policies such as education, social services, and health care and the private costs of taxation policy. Rose estimates that over three-quarters of public spending in OECD nations goes for the public provision of private benefits, with the single biggest program being social security (32% of public expenditures) followed by health and education (at 13% for each). Rose further estimates that over 90% of the British population, and a majority of U.S. citizens, receive a "major private benefit" from their government each year. Given such a large group of citizens who are beneficiaries of such a large portion of the public budget, it is difficult to understand the widespread public disenchantment with government. After examining public attitudes toward spending priorities and confidence in government, Rose concludes that "ordinary people today approve of government as a source of private benefits, but many doubt that government always spends money well when it provides collective public goods" (p. 25).

While Rose correctly interprets 1988 public opinion data indicating there is widespread sentiment among U.S. citizens for reducing expenditures on foreign aid, defense, space exploration, and public welfare programs, and for increasing expenditures for education, health, crime, and environmental programs, Rose is too rough in his categorizations of private benefits and public goods. For instance, there are substantial public benefits (externalities) of health, education, and environmental programs. Similarly, there are substantial private benefits from the provision of space exploration and defense programs. More microanalysis is required to disentangle the factors affecting the public support and nonsupport for this array of public policies.

A potentially rich contribution of *Ordinary People in Public Policy* is the examination of the components of individual consumption as the sum of goods and services produced in three different economies (the official economy of the national income accounts; the unofficial economy, or black market; and the domestic, or household, economy). Further, Rose defines total welfare in society as the product of benefits produced by markets, households, and government. This portion of the book is underdeveloped in that it does not explicitly relate back to the private consumption of education, health services, and other public policies; nor is it linked to the existing political science research on the coproduction of public goods by citizens. Had these two key ideas of the three economies and the three producers of aggregate welfare been presented earlier in the volume, the work would have been more coherent and theoretically more powerful.

This book is effective in two ways. First, given Rose's perspective of "the underall view of ordinary people" and his call for restoring a behavioral basis to the study of public policy, this book identifies a rich variety of topics deserving additional attention. Second, the comparative nature of the analysis is very useful. This book would be appropriate for an upper-level public policy course intended to expose U.S. politics students to a bottom-up perspective on politics with some non-U.S. examples.

DAVID J. WEBBER

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In Search of Leadership: West Bank Politics since 1967. By Emile Sahliyeh. Washington: Brookings Institution, 1988. 201p. $28.95 cloth, $10.95 paper.

In December 1987 the Palestinians living in the West Bank of the Jordan River and the Gaza Strip unleashed their *intifada* (uprising) against Israeli military occupation. Since then, a flurry of diplomatic moves and peace proposals have jammed the corridors of world chanceries, ending with U.S. Secretary of State Baker's plan. So far, this plan has not yet seen the light. One of the reasons is the crisis of leadership in the Jewish state. Emile Sahliyeh's book assesses the quest for leadership on the Palestinian side.

The book covers Palestinian politics in the West Bank from 1967 to 1987. Until 1967 local politics in the Occupied Territories were controlled by the traditional elites usually linked to the Jordanian monarchy. After the Israeli army occupation of the West Bank in 1967, new social and political forces emerged; and Palestinian nationalism, Marxism-Leninism, and Islamic fundamentalism became the three major ideologies contending for the allegiance of the Palestinians in the West Bank. Palestinian politics in the West Bank are also heavily influenced by the interests of Jordan, Israel, and the Palestine Liberation Organization (PLO). Sahliyeh writes that "the conflicting interests of those three have only served to deepen disunity and fragmentation among the ranks of the local elite" (p. 7).

The author has a first-hand knowledge of the various strands of leadership in the West Bank. Sahliyeh covers the rise and fall of the traditional elite, the emergence of the pro-PLO urban elite, the communists' quest for leadership, the student movement and student political activism before the *intifada,* and finally, the increasing role and influence of Muslim fundamentalist forces.

Between 1982 and 1986 West Bank politics were mostly defined by a balance of power between the pro-Jordanian traditional elite and the pro-PLO pragmatic politicians. The predicament of the Palestinians in the Occupied Territories is that they are a microcosm of fragmentation inside the Palestinian community and Arab politics in general. Palestinians in the West Bank, despite the *intifada,* are not yet masters of their fate. In 1988 King Hussein, perceiving that he had lost popularity among young Palestinians, severed all Jordanian claims to the West Bank. The history of Jordanian–Palestinian relations is a perfect illustration of a love–hate relationship. Throughout the turbulent years covered in this book one can witness the ups and downs between the Hashemite Kingdom and Arafat's PLO. As a result of Hussein's move, today the PLO and Israel compete for the control and destiny of the Palestinians living in the West Bank.

At the end of his book Sahliyeh defines the four factors determining the political behavior of West Bank elite groups. The first is defined by the geography and cultural ties of the Palestinians. Palestinians under occupation will have to determine their future in coordination with the Arab world and the PLO headquarters in Tunis; and Israel also has a major impact on how the future political shape of the West Bank will look. The second factor is defined by the absence of a viable economic base in the West Bank. Sahliyeh claims that "the shortage of indigenous economic resources will sustain the West Bank elite's economic dependency and vulnerability to outside pressures and manipulation" (p. 178). The third factor affecting the behavior of West Bank elite is illustrated by the intricate relationship of Jordan, Israel, and the PLO in setting the agenda of Palestinian politics. West Bank politicians have to endure frequent harassment by Israel's military, constant infighting between various factions of the PLO, and finally, the role and presence Jordan still has among traditional Palestinian politicians. The fourth factor is determined by the emergence of mass politics in the West Bank. Since the early 1970s and the creation of institutions of higher education in the Occupied Territories, students have played—and still play—a crucial role in determining their future. The *intifada* has dramatized the impact of the politicization of Palestinian youth.

An important factor only slightly dealt with by the author is a prescription for enhancing the dialogue between Israelis and Palestinians. Sahliyeh writes that the PLO has allowed a "controlled dialogue" between West Bank Palestinians and Israelis. Nonetheless, the PLO fears that "an open dialogue could so increase the credibility of those involved that they might act independently of the PLO" (p. 172). No peace will be possible if the fears and concerns of Israeli Jews have not been totally assuaged by the Palestinians. The Israelis will have to acknowledge or learn that the Palestinians as a people are entitled to the same human rights to statehood and freedom from military occupation as the Israelis claim for themselves.

Sahliyeh's book is well-written and thoroughly documented with primary sources, interviews, and the main literature on Palestinian politics in Arabic and English. It is must reading for scholars and students seeking a firm grasp of what is happening today in West Bank politics. As with any other book written on contemporary Middle Eastern developments, this one has already been overtaken by the events in the West Bank since 1987. Since the *intifada* began, several new factors have been brought into the Israeli–Palestinian picture: a significant change in West Bank population (70% now under the age of twenty-five); the deep crisis in Israeli leadership; the growing influence of Jewish and Muslim fundamentalist forces; and, finally, the new demographic reality brought about by the influx of Soviet Jews.

GEORGE E. IRANI

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Authoritarianism and the Crisis of the Argentine Political Economy. By William C. Smith. Stanford: Stanford University Press, 1989. 395p. $42.50.

With the recent reemergence of democratic government in Latin America, scholars and politicians alike have turned their attention to the dilemmas faced by newly elected heads of state. Severe economic crises, military insubordination and labor unrest underscore just how fragile these new democracies are. But according to William Smith, it is evident that the previous authoritarian regimes were equally fragile, unable either to stabilize or to legitimate themselves. At least, that is one of the central conclusions of the author's study.

Smith's object is to analyze why military regimes in Argentina have been unable to either consolidate or popularize their hold on power in order to complete what the author describes as the "restructuring" of society. The book, which is very well conceived and written, is largely successful in demonstrating just how important political considerations are in either facilitating or thwarting such restructuring.

On assuming office, military leaders frequently bathe in the initial euphoria of unbridled power and the prospects of transforming their visions of national unity into concrete policy. Believing that only the armed forces are capable of transcending sectarian interests to both embody and assert the national interests, they embark on messianic goals of security and development. Convinced that by virtue of their unique vantage point only they can hover "above politics," they eschew partisan politics in favor of rule by decree. The military's assumption that it reflects truly national (nonpartisan) aspirations is a naive one. But, as Smith asserts, the presumption that authoritarian regimes can indefinitely rule by administrative fiat without regard for fundamental political rules of behavior is equally naive. It is the strength of this book that it treats the *politics* of authoritarian rule not as epiphenomenal to economics but as primary in nature.

If in the short term military governments can govern arbitrarily and unilaterally by squashing dissent and prohibiting all forms of democratic contestation, in the longer term "politics, defined as the clash of competing interests and ideologies and the struggle to control the state and influence public policies, [proves] ubiquitous" (p. 72). As Smith eloquently demonstrates, the unifying goals of the regime come face to face with special interests, and resultant policies—irrespective of how broadly conceived they may be—produce both winners and losers. Even a powerful economic class, such as the Argentine pampa bourgeoisie, must on occasion succumb to defeat at the hands of powerful state technocrats who wish to impose their own agenda.

Fortunately, Smith does not fall into the trap of arguing that states lack autonomy and must therefore quickly reverse policy direction to mollify disgruntled capitalists. To the contrary, he explains how General Onganía's economic team, under the guidance of Adalberto Krieger Vasena, adopted an unorthodox stabilization program that stimulated demand through public sector expansion and in doing so "ignored the criticisms of influential economic groups" (p. 83). By accentuating the fact that state leaders have autonomy that they employ to defend their own interests and ideologies, the author has improved on earlier studies in which dominant classes always kept the state on "a short leash."

However, Smith also points out that state autonomy will prove illusory and policies will not be fully realized if the regime cannot construct a stable, hegemonic system with multiple bases of support. It is critical even for authoritarian regimes to consolidate as much political support as they can to head off opposition from injured sectors of the entrepreneurial class. It is equally critical that military rulers know how to ensure cohesion within their own ranks lest special interests prey on a divided military institution and thereby weaken the regime. General Juan Carlos Onganía, the leader of the military-sponsored Revolución Argentina, could do neither; nor could he subordinate the powerful trade union movement in order to gain labor compliance with his new political and economic programs. Despite the fact that Onganía's program did not prove especially injurious to the economic well-being of the workers, the government was unable to parlay its economic successes into trade union support for the regime because it had not given adequate attention to the political implications of its policies. Thus, the authoritarian regime could not stabilize or legitimate itself.

As a result of the failures of the Argentine Revolution of 1966–70 and the military Proceso (Process of National Reorganization, 1976–83), Smith concludes that all authoritarian regimes face virtually "insurmountable obstacles" in state-led restructuring. Here, the author is too quick to generalize from a single case. In Chile, for example, General Pinochet's relentless use of coercion and adroit manipulation of military promotions and retirements left his government practically unchallenged and able to effect dramatic changes in the economic system that will be felt for decades. In Brazil, where organized labor was more easily domesticated than in Argentina, the regime had an easier time of it as well, engineering enormous economic successes while subordinating or coopting its opponents. The author's analytical model has (as he had intended it) provided a useful framework which takes into account the "specificity and internal logic" (p. 5) of the case under review. Those who wish to gain insight into the political economy of Argentina would be remiss in not reading this book. But without a structured, country comparison, the lessons of Argentina cannot and should not be easily extended to the whole region.

DAVID PION-BERLIN

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French Caesarism from Napoleon I to Charles de Gaulle. By Philip Thody. New York: St. Martin's, 1989. 231p. $39.95.

Philip Thody's scholarly specialty—literature—explains in part, perhaps, both the virtues and weaknesses of this book. An authority on Camus, Sartre, Genet, Barthes, and other similar figures will surely bring a fresh view to the political history of France. However, the risk that is obviously run is that naïveté about what both political science and history have already learned about France will detract from any merits that a "fresh view" brings. Unhappily, this book runs that risk and, in the main, loses.

Thody contends that French political history is characterized by a paradox: the "most sophisticated country in the world [is] vulnerable to the rule of military men" (p. 10). This is French Caesarism—"the assumption of power at a moment of real or alleged national crisis by a figure owing his prestige to genuine or associated military achievements" (p. 15). The rest of the book is an attempt to describe French Caesarism through an examination of the four Caesars that Thody identifies: Napoleon I, Napolean III, Marshall Pétain, and General de Gaulle.

Thody concludes that French political life has been extremely weak for centuries and that the French have spent much more of their energy fighting each other than in uniting for common political purposes but that this syndrome has been broken by the durability under duress of De Gaulle's Fifth Republic. Indeed, Charles de Gaulle is clearly Thody's hero. Because he voluntarily gave up power in 1946 after a glorious World War II resistance record and because he founded a regime that withstood both is own political misjudgments and the social upheaval of the 1960s, De Gaulle is the Caesar who, for Thody, definitively saves France from Caesarism.

This is, to say the least, a large claim about the Fifth Republic, which both political scientists and historians might treat with a large dose of "wait-and-see." One ought never forget in treating France that the ancien *régime—nurtured to great but imperfect glory* by Louis XIV, Mazarin, Colbert, and countless others—fell in 1789 after two centuries of relative stability (as these things go).

Moreover, scholars of contemporary France know only too well that the Third Republic constituted the longest period of relatively democratic politics that France has ever known. Yet Stanley Hoffmann in "Paradoxes of the French Political Community" (1963) underscores the dangers of *immobilisme* in a regime of notables. This immobilism, argued the great historian Marc Bloch in *Strange Defeat* (1948), played a primary role in the humiliating rout of France by the Nazis in 1939. Thody does not discuss these arguments.

One could go on by citing a number of influential works about postwar France, in both history and political science, that belie Thody's view of French Caesarism. Alain Peyrefitte's *Le mal français* (1976), for example, made quite an impact on French studies in general with its diatribe against the French national character, which Peyrefitte took to be obstreperous, cantankerous, counterproductive, and self-destructive. Laurence Wylie, in *Village in the*

Vaucluse (1957) showed Provençal villagers to be deeply suspicious of central authority but also highly dependent on it.

Clearly, there is something unique to the development of French political culture. Compared to Anglo-Saxon democracies, there has been, of course, greater instability and more volatility in political regime type. But perhaps the Anglo-Saxon lens is not the only one—or even the best one—to allow us to look at France. Thody uses Caesarism, in fact, to point up the deep discontinuities of French politics. But he ignores the continuities, which in the end may be more important. Throughout the modern era, France has been characterized by strong, centralized, rationalized administrative structures. These have provided a stable foundation that has underlaid the superficial instabilities of French politics. As Tocqueville observed, neither the revolution nor Napoleon razed the administrative edifice of the ancien régime. Rather, he argued, they each built on it, expanding its capacities and turning it to more modern purposes.

Thody does tell a number of interesting stories about his protagonists, dwelling on both large and small details. He has read widely in various literatures of political science, history, literature, art, and sociology; but unfortunately, his reading has been unsystematically selective. Furthermore, as his tales unfold, they are marred by periodic silliness.

For example, while in the midst of discussing Napoleonic civil reforms and evaluating their effectiveness and their efforts on the class structure of French society, Thody suddenly drops a comment into the story to the effect that Napoleon "inaugurated . . . the pattern whereby it is the leader who comes from just outside the country who proves to be the most enthusiastic nationalist" (p. 40)—or, "The aggression which [Napoleon] showed in trying to take his elder brother's place went far beyond any rational bounds, and eventually proved his total undoing" (p. 41). Such lapses mar each of the four accounts of Caesarism. While Thody claims that it is to his advantage to be an "amateur historian" becuase it is the amateur who is "free to speculate," his lack of intellectual rigor tends to vitiate this claim.

The virtues of the book, however, are that it does constitute a good read by a fireplace in the winter during a weekend when one wishes to escape a bit into some casual political history that does succeed in raising an interesting question or two. Just do not mistake it for the definitive statement on either Caesars or France.

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The Great Transition: Political and Social Change in the Republic of China. By Hung-mao Tien. Stanford: Hoover Institution Press, 1989. 324p. $22.95 paper.

In the past several years, Taiwan has begun a process of democratization. Martial law has been lifted, opposition political parties have been legalized, the press has become more boisterous, and electoral competitiveness has increased. What are we to make of these changes? Hung-mao Tien's detached and detailed analysis views Taiwan as "a modernizing authoritarian regime with strong characteristics of a one-party system in transition toward a dominant-party system" (pp. 12–13). Tien thus recognizes the significance of political reform on Taiwan without jumping to overly optimistic conclusions. Throughout the book he points out the fragility of democratization and its limitations.

The book is organized into 10 chapters. The first provides some historical and comparative context to Taiwan's political development. Eight chapters follow on various aspects of the island's recent political changes: economic and social modernization, interest groups, the party system, the government, representative institutions, electoral politics, the mass media, and foreign relations. The brief conclusion discusses how both international and domestic forces have helped nudge the Nationalist party (KMT) toward reform. The bibliography is excellent, citing a wealth of primary and secondary Chinese language sources as well as most important English language analyses of Taiwan politics. An appendix includes Taiwan's constitution and the "Temporary Provisions Effective during the Period of Communist Rebellion," which supplants important sections of the constitution.

The great strength of the book is Tien's insightful political and institutional analysis. He explicates the Leninesque structures of power that have bolstered KMT hegemony. Although some of the most noteworthy reforms have centered on establishing regular competitive elections for the Legislative Yuan, the national parliament, Tien reminds us that only a minority of seats, about one-fourth, are open to such challenges (p. 146). The majority are reserved for superannuated politicians elected to their positions in 1947. This is the legacy of the KMT claim to represent all of China that is used to legitimate continuing domination of the legislature, even if it is no longer de facto foreign policy. Tien also analyzes extraconstitutional organizations that similarly bolster one-party rule, such as the Garrison Command and the National Security Council (pp. 108–12). Tien's focus on institutions points to specific structural reforms needed to deepen Taiwan's nascent democracy: scaling down the power of the president, reducing the role of the National Assembly (if not abolishing it altogether), redefining extraconstitutional organizations, and opening more legislative seats to competitive elections.

Tien is also very good in his discussion of less formal processes, especially factional politics. He traces intra-KMT factions to their roots on the mainland and suggests how they have shaped national legislative politics (pp. 148–51). He shows how factions are manifest in election campaigns, casting light on local-level politics (pp. 164–71). In addition, Tien surveys other key political forces: the rise of the opposition and the continuing political significance of ethnic Mainlander-Taiwanese differences.

The primary weakness of the book is its theoretical context. Although Tien cites some of the recent work on transitions to democracy, he constructs his analytic categories largely from political development literature of the 1960s and 1970s. More thoroughgoing consideration of the newer work, most notably A. Schmitter and colleagues' *Transitions from Authoritarian Rule* (1986), would have provided a stronger basis for analysis and comparison. In this regard, the repression of labor and the impossibility of a Left politics in Taiwan—both of which Tien notes—takes on greater significance. They distinguish Taiwan's transition from the Latin American experience. Also, contemporary comparative studies of democratization could contribute to our understanding of the impact of international forces on Taiwan's politics (which Tien reviews) and the role of the military (which Tien mentions but does not fully analyze).

All in all, this is a very good book. Tien's argument is carefully drawn and well supported: democratization in Taiwan is genuine but not guaranteed. Moreover, this is likely to be the best comprehensive study of Taiwan's political institutions for some time to come.

GEORGE T. CRANE

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INTERNATIONAL RELATIONS

The Rockets' Red Glare: When America Goes to War—The Presidents and the People. By Richard J. Barnet. New York: Simon & Schuster, 1990. 476p. $24.95.

Barnet poses one central question: What differences have the people made in the conduct of U.S. foreign policy—particularly with respect to issues of war and peace? To address that question, he reviews the history of American foreign policy since the establishment of the Republic.

Barnet notes that U.S. leadership has often viewed public opinion as wrongheaded. He quotes a declaration of Dean Acheson that if you did what the people wanted, "you'd go wrong every time" (p. 15). In general, Barnet argues that leaders worry about public opinion but seldom let it constrain presidential action. Instead, leaders sell their policy preferences and decisions to the public.

That assessment troubles Barnet, because he believes the process results in a failure to discuss interests publicly and to benefit from ideas originating outside of a small circle around the president. Barnet argues, for instance, that after the United States became vulnerable to Soviet counterattack, the public quickly recognized the suicidal character of threatening first use of nuclear weapons and began to oppose such a policy; but the leadership did not change the policy. Moreover, after both sides built substantial nuclear capabilities, the public saw better than the leadership that a continued arms race posed a greater threat to the country than did the USSR. Today, Barnet feels that the public understands that the critical issue facing the country is saving the planet (both from nuclear destruction and environmental damage) and has the ability to help develop appropriate national responses to that threat, were the leadership to encourage a serious public dialogue.

The historical discussion of the book focuses primarily on one link in this chain of argument—the uneasy relationship between the public and its leaders. The discussion begins by detailing disagreements among the founding fathers on an appropriate response to the French Revolution and the impetus that gave to the formation of parties; it ends by discussing what Barnet characterizes as cynical but highly professional efforts by the Reagan administration to manipulate and circumvent public opinion. Barnet argues that the gap between leaders and public has widened in recent years.

The book fills a substantial hole in the literature. The approach is historical narrative, but Barnet cites and understands a good sample of the modern social science literature on the linkage of public opinion and foreign policy. Most of his period, however, lies before public opinion polling (which he does not cite heavily even in recent years). Instead of polls, he relies on impressively detailed attention to the press, to formation and growth of mass movements and specialized organizations, and to reports (often autobiographical) on leadership perception of public opinion.

He solidly substantiates the thesis that the linkage between public and leadership has been uneasy. In doing so he provides those interested in that linkage with a wealth of anecdotal information and some real insights into the changing character of the relationship across administrations and eras.

But his argument is a broader one and includes claims that the public can and should play a more active role in policy formation. Even readers sympathetic to the argument may find his evidence in support of these claims uncertain. Barnet's own reports on the extreme public pressure to demobilize quickly after World War II and on the excesses of anticommunist sentiment in the McCarthy era cast some doubt on his position. Barnet would argue, however, that leadership brings most such problems of mistaken public judgment on itself: during the war, it "protected" the public from information on the cracks in the alliance with the Soviets and it then purposefully inflamed the frenzy of anticommunism to counter demobilization pressure.

There are other weaknesses in the volume. The close attention to domestic interactions sometimes assumes a knowledge of the critical external environment that not all readers will share. That may be an unavoidable penalty for the richness of detail Barnet provides on leadership and public interactions. But Barnet's failure to draw summary conclusions from the chapters and sections of his historical analysis is impossible for a social scientist to excuse. The overall argument is elaborated in the introductory and concluding chapters, but Barnet fundamentally leaves for the reader the exercise of relating the narrative to that argument.

No book will ever conclusively resolve the question of appropriate balance between public input and leadership flexibility in the process of foreign policy formulation. Barnet provides a refreshing, highly readable, and informative contribution to the debate. It is a very good book.

BARRY B. HUGHES

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Britain, NATO, and Nuclear Weapons: Alternative Defence versus Alliance Reform. By Ken Booth and John Baylis. New York: St. Martin's, 1989. 374p. $55.00.

NATO Strategies and Nuclear Weapons. By Stephen J. Cimbala. New York: St. Martin's, 1989. 276p. $45.00.

The political and economic crises that have overwhelmed the Soviet Union and Eastern Europe since 1989 make it extremely difficult to write and publish in a timely manner on the issues of NATO strategy and nuclear weapons.

Much of the debate within NATO about the value of nuclear weapons for deterrence and defense is premised on the existence of a serious Soviet conventional military threat to Western Europe. Events of the past year have made the discussion of nuclear issues within NATO even more difficult than before. As the perception of a Soviet military threat to the West declines, it will become increasingly difficult to justify a strategy based on nuclear deterrence requiring the presence of large numbers of nuclear weapons in Europe.

The challenge to the authors of both of these volumes is to aim at a moving target. Each book addresses a piece of the puzzle, and together they serve to enlighten our understanding of nuclear strategies and weapons in NATO. Some of the material has been overtaken by political events of the past year. This fact should not detract, however, from the valuable insights the authors offer about the domestic foundations for NATO nuclear strategies and weapons (in the case of the Booth and Baylis volume), or the role of nuclear strategies and weapons for the processes of escalation and war termination (in the case of the Cimbala volume).

In *Britain, NATO, and Nuclear Weapons* Ken Booth and John Baylis examine the arguments for and against an "alternative defense" posture for Britain that does not rely primarily on nuclear weapons. Booth takes the lead, in the first two-thirds of the book, in arguing that nuclear weapons have become dysfunctional for the defense of Europe. The decreasing Soviet military threat and the "political window of opportunity" (p. 219) opened by Soviet president Mikhail Gorbachev have created a situation in which NATO's doctrine of flexible response and the concept of limited nuclear options are incredible. An alternative defense strategy for Britain, in Booth's view, should be based on the concepts of nonprovocative defense and defensive deterrence. The shift away from an offensive strategy will prompt a positive response by the Soviet Union. For Britain, the advantages are clear—decreasing reliance on nuclear strategies and nuclear weapons means that Britain becomes less of a target for Soviet nuclear weapons and that scarce defense resources will be made available for conventional force improvements.

Baylis presents a counterargument. Although nuclear deterrence is not without risks, there are serious political and military weaknesses in a policy of nonnuclear defense—for both Britain and NATO. In particular, Baylis highlights the potential for nuclear blackmail in a situation in which NATO has formally and publicly accepted a policy of "no first use" of nuclear weapons. Baylis does believe that some reform of NATO strategy is necessary but that reform must be evolutionary, not revolutionary. Over the long term, NATO should decrease its reliance on nuclear weapons and formulate defense plans that are not provocative. But this shift does not require—nor should it require—a commitment to nonnuclear strategy in the near term. After all, Baylis argues, nuclear weapons have kept the peace for more than 40 years.

Stephen Cimbala is one of the more prolific writers on the subjects of nuclear strategies and weapons. This volume includes some material that has been published elsewhere but also contains original pieces on the subjects of escalation and war termination in the NATO context. Cimbala discusses a wide range of issues: technology, Soviet "New Thinking" and NATO strategy, prospects for a conventional defense of Europe, extended deterrence, nuclear escalation, nuclear surprise, flexible targeting and NATO command systems, U.S. maritime strategy, SDI and the defense of Europe, and war termination in the European theater. Of particular interest are the chapters on U.S. maritime strategy and SDI—issues generally not discussed with this degree of sophistication in volumes on NATO strategies and nuclear weapons.

The Cimbala volume clearly is the more sophisticated and thought-provoking of the two. He deals with extremely complex issues and offers no easy answers—and there aren't any. For example, the problem of deterrence and low-intensity conflict remains a core issue for the United States and its European allies. Strategies and forces that deter the Soviet Union or other major powers often fail to deter the actions of revolutionary movements or terrorists. The volume does suffer from the problems associated with collections of writings done over a period of time. Neither relationships between the issues discussed nor transitions between chapters are always clear. The volume is obviously written for an audience already familiar with the basics of NATO strategies and nuclear weapons.

The Booth and Baylis volume, in contrast, is both simpler and more readable than the Cimbala effort. The concepts of nonprovocative defense and defensive deterrence are widely known within both the academic and policymaking communities in Europe and the United States, and they appear in much of the public debate about security issues in Europe as well. This volume would have greater balance if less space had been devoted to the Booth contribution and more to the Baylis response.

In short, both of these volumes offer important insights into the debate about nuclear strategies and weapons in NATO. Cimbala writes largely for the expert, while Booth and Baylis write for a broader audience. Despite the events of the past year—or perhaps because of them—the future of nuclear weapons in NATO remains a critical issue for the alliance. These books will make an important contribution to the continuing debate.

LINDA P. BRADY

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Success and Failure in Arms Control Negotiations. By April Carter. New York: Oxford University Press, 1989. 308p. $55.00.

This is a comprehensive and detailed study of primarily U.S.-Soviet arms control negotiations from the test ban talks of the late 1950s through the Strategic Arms Reduction Talks (START) of 1989. Carter's main objective is to elucidate the factors—political, economic, technological, and psychological, among others—that have presented obstacles to arms control, as well as the factors that have, or could, contribute to success. Beginning with a résumé of virtually all factors that might oppose or promote arms control, she seeks in subsequent chapters to see which of these were operative in detailed case studies. The cases include the test ban talks of 1958–63 and the Comprehensive Test Ban (CTB) negotiations of the 1970s; Strategic Arms Limitation Talks (SALT I and SALT II); the Intermediate-Range Nuclear Forces negotiations (INF) and START from 1981 to 1989; and multilateral talks in Europe, primarily the Mutual and Balanced Force Reduction negotiations (MBFR). One might look at the history of superpower arms control as a case of a glass one-tenth full (or, alternatively, nine-tenths empty). Carter's view is decidedly the latter. Not surprisingly, one of her main findings is that potential obstacles to arms control far outnumber, and generally outweigh, factors conducive to success.

One of the major reasons for this is that neither superpower has pursued arms control as a goal in its own right. Thus, for example, at times states have entered arms control negotiations with the objective of gaining some unilateral advantage. Talks may be initiated to gain diplomatic advantage and prestige or to protect weapons programs from attack by a hostile Congress or public opinion. They may be designed to assuage or to pressure allies. On the more positive side, arms control talks may have the goal of promoting détente, even if not the reduction of weapons.

Even when leaders of each superpower genuinely desire some form of arms control, Carter shows there are major potential obstacles. The arms lobby and its powerful allies can slow or even block agreement. At times these domestic forces can extract major concessions, whereby arms control will actually promote the arms race by channeling weapons development into new areas. Technological developments can lead to new problems of verification (e.g., MIRV, cruise missiles, "decoupling" nuclear tests), making agreement harder to achieve. Indeed, perhaps the most significant failure of arms control to date has been the failure to limit technology in any serious way, thus paving the way for future weapons development. Carter's discussion of these and numerous additional obstacles suggests what might be called the iron law of arms control: to succeed, political conditions on *both* sides must be overwhelmingly conducive; opposition by significant forces on *either* side will produce failure. Her discussion also suggests a paradox: peace movements have probably been the main force leading to arms control negotiations; yet as negotiations move toward success or the appearance of success (often on very limited measures), this undermines the conditions for further success by demobilizing peace movements.

One of the most useful contributions of Carter's book are the detailed case studies, which recount not only the history of negotiations but stress the broader political context and how this affected their course and outcome. The history is based on a synthesis of a large number of secondary sources. Also of interest is the discussion of the significance of negotiating strategy for the outcome of arms control talks. Carter argues that two favorite strategies of conservatives—"negotiating from strength" and the building of weapon systems as "bargaining chips"—are more likely to undermine than promote arms control. Furthermore, she argues that contrary to the perceptions of some, these strategies do not account for the recent progress and optimism in the arms control arena.

Less satisfactory is the attention to numbers. Believers in arms control hold that numbers of weapons matter a great deal, yet in the cases presented here one is hard-pressed to find useful, succinct summaries of the superpower arsenals or indeed in some cases what the actual negotiations were about in terms of numbers. This shortcoming is likely related to a less explicit theme in Carter's book, namely, that in an age of massive overkill and massively redundant weapons, numbers—as conceived in both military and traditional arms control thinking—may, indeed, not matter very much either in terms of reducing the likelihood of war or in giving either side anything approaching a usable "strategic advantage." The latter view is argued persuasively in another recent book, William A. Schwartz and Charles Derber's *The Nuclear Seduction* (1990). Thus, there is a tension in Carter's book between the idea that arms control is good and matters and the idea that arms control can only be meaningful in the context of far different concepts of security and radical reductions in armaments. With the possible end of the Cold War on the horizon, such prospects appear brighter. Yet at present, her book is an important, if depressing, reminder of how much talk and effort have been expended to get not very far at all.

GREGORY MCLAUCHLAN

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Nuclear Endings: Stopping War on Time. By Stephen J. Cimbala. New York: Praeger, 1989. 295p. $47.95.

Few people, in the course of their daily lives, like to dwell on the horrible possibility of a nuclear war. Yet the politics of the early and mid-1980s, the Reagan defense buildup, arms control stagnation, the strategic defense initiative, nuclear war-fighting strategies, and nuclear winter forced most of us to reconsider, debate, and analyze the grisly possibilities. It is in this context, rather than the rapidly changing nature of the superpower relationship and the political landscape of Central and Eastern Europe from 1989 to the present that Stephen Cimbala's analysis is best assessed. As long as the possibility of nuclear war exists, Cimbala argues that it is certainly sensible to analyze and plan for nuclear war termination. Even if the probability of nuclear war is very small (and presumably getting smaller and smaller), it is better to develop and implement strategies and plans for terminating nuclear wars as quickly and with as little damage as possible than to be totally unprepared for such a circumstance. Readers may find some of Cimbala's other reasons for the study of war termination (improved strategic war planning and enhanced deterrence) more analytically and politically controversial. Readers who believe that any weapon, strategy, or plan that makes nuclear war fighting more likely because it may be perceived to be less dangerous and costly will probably find little to persuade them in Cimbala's analysis.

War initiation has been the subject of far more scrutiny than war termination. We know much more about why and how conventional wars start than how they stop. It is obviously not because we have many empirical cases (unlike the nuclear war situation) but rather because understanding and helping prevent wars from starting has been perceived to be more important than understanding and helping them end. Not only is there more historical and thick-descriptive work on war initiation, but there is a far richer and more developed theoretical tradition than that connected with war termination. Thus, Cimbala has fewer building blocks to borrow to begin construction of a theory of nuclear war termination. As a result, in the introductory chapter on war termination, he cobbles together a set of conceptual issues and processes related to conventional war termination, crisis escalation and management, and coercive diplomacy. He does a nice job using these concepts and processes to explicate the logic, problems, and possibilities of nuclear war termination. The concepts of "agreed battle," escalation control, and coercive diplomacy are examined in the necessarily abstract what-if world of nuclear war termination. Cimbala, by necessity, also employs a series of nonnuclear historical examples of these processes to draw out a number of relevant points. Not surprisingly, U.S. experiences in Korea, Vietnam, and the Cuban Missile Crisis constantly appear as well as the classic case of World War I. Cimbala shows that nuclear war termination requires many aspects of the more studied problems of crisis management and the management of limited wars; tacit agreement on bounds by participants, limited goals, communication capabilities, and correct perceptions of costs and escalatory dangers. Cimbala shows that, as events are studied from the war termination perspective, there is a substantial spillover to other conceptual issues in strategy and deterrence.

Surprisingly, even with the paucity of theoretical building blocks, Cimbala makes no concerted effort to organize a theory of nuclear war termination. This is unfortunate, since a theory of nuclear war termination could be linked to any one of a number of rich formalisms or theoretical traditions such as decision theory, game theory, bureaucratic politics, decision making in times of stress, misperception, or recent work on the evolution and maintenance of cooperative behavior (regimes)—that others have employed to study crisis management, the management of limited wars, and strategic theory in general.

Following the introductory chapter on war termination, the remaining chapters of the book examine standard issues in nuclear strategy with a specific focus on war termination. These include, the balance of terror, command control communications and intelligence, strategic surprise, extended deterrence, Soviet views on war termination, and strategic defense. While there is not much new here, Cimbala does a very good and thorough job analyzing these issues as they relate to war termination. Each chapter provides an excellent synthesis and summary of the recent literature on these important topics. One can hardly fault Cimbala for failing to predict the extraordinary changes in international politics in 1989 and 1990. However, it does make some aspects of this part of the book a bit dated. This is particularly true of his discussions of Soviet views of war termination, extended deterrence, and U.S. and NATO options in Europe.

Cimbala correctly points out that nuclear war termination is a neglected subject. His contribution certainly helps bring this important subject to our attention. While not theoretically rich, *Nuclear Endings* provides an excellent summary of the relevant literature and an interesting synthesis and application of relevant concepts extracted from crisis management, coercive diplomacy, and crisis escalation to the nuclear war termination problem. Cimbala admonishes those who would choose to neglect this problem by pointing out how badly things might go if a nuclear war were to start and there were no serious or careful plans for its termination. However, while Cimbala elaborates some general guidelines and possible scenarios for nuclear war termination in his concluding chapter, neither concrete plans nor a series of specific *dos* and *dont's* are provided. Lastly, Cimbala never really challenges the basic presumption of his analysis, namely, that studying, developing and implementing plans to terminate a nuclear war makes its outbreak no more likely and its termination easier. It is a presumption that others do not take as given.

STEPHEN J. MAJESKI

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Universal Human Rights in Theory and Practice. By Jack Donnelly. Ithaca, NY: Cornell University Press, 1989. 295p. $36.50 cloth, $12.95 paper.

Every once in a while a book appears that treats the leading issues of a subject in such a clear and challenging manner that it becomes central to understanding that subject. Jack Donnelly's *Universal Human Rights in Theory and Practice* is just such a book. It should become required reading for all specialists and advanced students interested in internationally recognized human rights.

Those familiar with Donnelly's other publications in this and other journals will have read much of this book already. Paradoxically, that does not detract from the value of the book. What Donnelly has done is pull together many aspects of international human rights that he has examined previously into a large and mature statement, revising and adding new material so as to produce an important work. The breadth of his treatment is especially impressive, truly covering both theory and practice.

There could be no simple thesis to a subject so broad and complex as international human rights. In general, what Donnelly shows (persuasively to me) is that there can be universal human rights in a multicultural and multinational world, despite the Western origin of the concept. He also shows that the process of implementing rights entails judgments about priorities and contingencies that nevertheless, do not detract from the principle of universality in terms of original conception. Finally, he argues with considerable evidence that foreign and international action in support of human rights will be secondary to action within each nation.

His interpretations are clear and argued with zest and stimulate critical thought. Donnelly conceives of human rights as *moral* rights, inherent in personhood and not dependent on public authority. They are rights possessed only by individuals, not by collectivities, such as peoples or nations. He consequently argues that "human rights claims thus are essentially extralegal" (p. 14). This "analytic" theory (p. 21) of the origin of rights turns out to be, as acknowledged, "controversial" (p. 21) and, indeed, *empty* (p. 23). The agreements and practices of public authorities are necessary to put flesh on his skeleton, and Donnelly accepts the 1948 Universal Declaration of Human Rights, in toto, as a reasonable indication of what internationally recognized human rights should be in contemporary times.

Although mostly agreeing in substance with Donnelly's conclusions, I wonder whether it is so important to develop an analytic theory of moral human rights if the content is nonexistent. If public authority, especially state policy as recorded in international organizations, still essentially defines internationally recognized human rights *for practical purposes*, it might make more sense to focus one's attention there. Donnelly's analytical theory only *appears* to bypass the moral or philosophic interpretations that are "notoriously controversial, and . . . intractable" (p. 21). His endorsement to the Universal Declaration implicitly refers back to precisely such moral or philosophical judgments. The inescapable fact is that by referring to human rights as moral rights, one puts the basic concept in the realm of controversial and intractable discourse.

Donnelly moves on to defend the Universal Declaration, and the two treaties that it spawned, Civil and Political Rights and Economic and Social (and Cultural) Rights, as constituting an appropriate core of contemporary human rights. This he does on the basis not of basic human need but of what is necessary for a life of full human dignity. While in theory he dismisses the notion that some of the 30 rights noted in the Universal Declaration are morally more basic than others, he acknowledges that the practice of states may require prioritizing attention to only certain rights. He writes of the need for "sound theoretical guidance" (p. 44) for such judgments, but is not clear from where or what such a theory can be derived.

If, like Donnelly, one accepts the first generation of rights (civil and political) and also the second (economic and social), while rejecting the third generation of solidarity rights (right to peace, safe environment, development, common heritage, etc.), on what basis is this done? If it be agreed that malnutrition deprives of human dignity as powerfully as torture or summary execution, how does advanced depletion of the ozone differ? We are back to the realm of controversial and intractable moral discourse. I prefer an analysis based on empiricism. I object to the right to development not because it is a third-generation right or a collective right but because it has no known content, no specified rights and duties, as acknowledged even by those states voting for the concept at the United Nations. The practice of political actors, not analytic or moral or any other theory, determines the content of rights.

Donnelly is most persuasive, in my opinion, in analyzing the relationship between human rights and cultural relativism and between human rights and economic development. His case study of culture and the caste system in India is enlightening. Particularly important is his dissection of the argument that suspension of human rights is required by the exigencies of economic development in the Third World. He convinces me that most violations of human rights, beyond the take off stage for economic growth, have more to do with elite privilege and profit than with the requirements of sustained growth, industrialization, and modernization.

Finally, he reproduces his excellent work on international human rights regimes as background to an examination of the role of national foreign policy in helping to implement rights. He arrives at the conclusion, widely supported by an increasing array of scholars, that foreign actors can mostly help implement rights indirectly, while direct protection must remain the domain of states and forces acting within states. Thus, whether one speaks of the United Nations or U.S. foreign policy, priority of action will be located elsewhere.

An excellent bibliography closes this readable, thoughtful, and provocative book. Donnelly is to be commended for focusing on a series of major questions, addressing them clearly and systematically, and approaching complex issues creatively.

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Arms Race Theory: Strategy and Structure of Behavior. By Craig Etcheson. New York: Greenwood, 1989. 245p. $39.95.

Lewis Fry Richardson's models of arms race behavior are elegant and mathematically rigorous, but they perform poorly when tested against arms expenditure data. Several scholars—Craig Etcheson among them—have tried to explain why. Etcheson suggests that researchers in the Richardson tradition have misconceived the nature of international interaction and need to account for policy makers' intentions in theories of arms accumulation processes. He argues further that analysts can find evidence of these intentions in a nation's force structure. In fact, he claims, the relative strengths of land, air, and naval forces reflect a nation's overall strategy and affect its competitive interactions with adversaries. Studies that rely only on defense budgets overlook specifically *strategic* interaction because aggregate data reveal little or nothing about force structure. Etcheson argues for computational models that incorporate strategic planning and develops an arms race simulation to illustrate the potential of such models for future theory building and research.

Hence, the author aligns his work with a loose but increasingly influential community of scholars dissatisfied with the mechanical nature of the Richardson models. A genuine strength of the book is Etcheson's balanced treatment of both the Richardson tradition and newer approaches. He criticizes the arms race literature without minimizing its accomplishments and points to the potential contributions of computational modeling without overstating what it can do. Moreover, he acknowledges the deficiencies of the arms race simulation he developed for the book and indicates clearly how it could be improved.

The book shows some of the strengths and weaknesses of a manuscript prepared for a specialized audience. The literature review and the discussion of analytical methods are easily the strongest chapters in the book. Its survey of the Richardson literature is comprehensive, thorough, and illuminating. It is an excellent synthesis that needed to be done. Likewise impressive is the author's discussion of methods for analyzing arms accumulation processes, where he handles difficult epistemological issues competently and engagingly. Here Etcheson makes a good case for the potential of analytical techniques inspired by research into artificial intelligence. Since a great deal of work in this area (computer chess, for instance) has explored connections between strategic planning and competitive interaction, this field of research suits Etcheson's purposes well. He argues that if cognitive process models sensitive to variations in strategy begin to supplant mechanistic models of international interaction, the next generation of research into competitive arms acquisitions could achieve substantial theoretical advances.

The book's two notable weaknesses arise from the somewhat limited scope of its original contribution. After an intellectual tour de force in the first three chapters, Etcheson barely develops the key concept of strategic planning in his computational model. Strategies in the model are tied to a state's force structure and are labeled *power projection, invasion force, continental defense, ocean control,* and *air-land battle.* Etcheson acknowledges that inferring strategic intentions from force postures in the way stipulated by his model implies a crude understanding of strategy. In addition, his notion of strategy is one of the simplifying conceptual moves that makes the model work. Still, Etcheson's presentation would benefit tremendously from a more extended discussion of the strategic conceptions built into his model. Then the reader would have a richer sense of the theoretical payoffs possible in this mode of analysis.

The book's second shortcoming springs from Etcheson's cursory analysis of his simulation's results. He limits his analysis to a single run of a hypothetical arms race between Melos and Delos (from Thucydides' *History of the Peloponnesian War).* Multiple runs of the simulation would demonstrate its usefulness more convincingly. Some preliminary hypotheses ought to be drawn from the enormous literature on arms accumulation, some of which deals with how military planning affects force structure. Then the arms race simulation could be tested under a variety of initial conditions, within specified strategic environments of theoretical interest. Reporting the results of every run would become tedious; but one wants to know in general terms how the other runs Etcheson conducted turned out, how the results compared with each other, and what could be learned from such a comparative analysis.

In sum, Etcheson's book on arms race theory evaluates the accomplishments of the Richardson tradition and assesses the possibilities for further research in this area. As analysts plan for the future of arms accumulation research, the question of what, if anything, should be preserved from previous stages of research in the Richardson tradition will be an important issue. Etcheson has clearly given a good deal of thought to this problem yet leaves the reader uncertain as to how radical a break with the Richardson tradition he wants to make. He argues unequivocally that older methods and models misconceived the nature of arms accumulation processes but also implies that to repudiate the earlier research programs entirely would be a mistake. However that may be, Etcheson's work will be of interest both to researchers in the Richardson tradition and to those exploring alternate modes of analysis in artificial intelligence and computational modeling. The discussion of the Richardson literature and of methods appropriate for analyzing arms accumulation are by themselves worth the fairly substantial price of the volume. The book integrates previous work very well, points toward intriguing possibilities of computational models of arms accumulation, and represents a valuable contribution to arms race theory. It ought to serve as an impetus to further work in the field.

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The American Conception of Neutrality after 1941. By Jürg Martin Gabriel. New York: St. Martin's, 1988. 289p. $45.00.

East-West Conflict and European Neutrality. By Harto Hakovirta. Oxford: Clarendon, 1988. 291p. $59.00.

At the outbreak of World War II, there were 20 states in Europe whose international positions and foreign policy orientations classed them as neutral. The United States, owing to its isolationist tradition and recent antiwar legislation, was itself a neutral country. By the end of the war, there were but five remaining neutral states in Europe; and the United States had taken the lead in forming a new collective security organization, the United Nations. Subsequently, it helped to establish the North Atlantic Treaty Organization and collective defense arrangements for other regions. Neutrality, therefore, seemed not only illegitimate but also historically passé.

This was confirmed by Secretary of State John Foster Dulles in a speech in Ames, Iowa on 9 June 1956: "These treaties abolish, as between the parties, the principle of neutrality, which pretends that a nation can best gain safety for itself by being indifferent to the fate of others. This has increasingly become an obsolete conception, and, except under very exceptional circumstances, it is an immoral and shortsighted conception."

These two books, by Swiss and Finnish scholars, describe the remarkable comeback that the classical concept of neutrality has made. Jürg Martin Gabriel, after giving a lucid conceptual analysis of U.S. neutrality practice from 1793 to 1941, applies his historical understanding to the critical transitional period, 1941-56. It is the United States' "conception" of neutrality that interests him, not so much any residual neutral elements in U.S. foreign policy itself.

This U.S. conception he finds manifested in the government's conduct toward the remaining neutrals, particularly those in Europe. In comparison with the belligerent actions of the Axis powers, U.S. wartime military behavior toward the neutrals was "nearly perfect" (p. 42). The United States respected their physical sovereignty and afterward paid compensation for any damage done—for example, by airplanes that inadvertently bombed, or that crash-landed on, neutral territory. In doing so, Washington went well beyond the strict obligations of the 1907 Hague Conventions, which did not treat air warfare.

In the economic sphere, however, the United States was far less respectful of the neutrals' independence. Switzerland was effectively denied the ability to maintain *courant normal* (i.e., prewar trade levels) with next-door neighbor Germany. U.S. officials virtually coerced that country into severely reducing its exports to Germany and into stopping the through-transit of German coal to Northern Italy. They also interfered with Switzerland's administrative integrity by refusing to recognize transfers of title of Nazi-looted gold or other such assets sent for "safe haven."

This intrusive attitude, Gabriel persuasively argues, carried over into the measures of economic warfare—the Mundt Amendment, the Coordinating Committee, and the Battle Act—that the United States used against trafficking with the Soviet Union after the Cold War started. Neutrals, like the NATO allies, were expected to be "supporting" states of the Free World's containment policy. In its Cold War behavior, the United States seemed to be ignoring its own heritage as a small, harmless, trading republic and also its liberal doctrine that commerce is inherently peaceful. As Gabriel comments, "Perhaps free international trade was, after all, a force working for peace as the Founding Fathers once believed. Was that contention totally without merit?" (p. 128).

Some allowances, gradually, were in fact made by the U.S. government for the postwar existence of neutrals, and pragmatism crept into U.S. policy toward them. In order to settle the Korean War, for example, it was necessary to find suitable members of an "impartial" commission to supervise repatriation of prisoners. Neutralist India's willingness to serve as a kind of superneutral "umpire" (along with the traditional neutrals Sweden and Switzerland and the "communist neutrals" Poland and Czechoslovakia) however immoral such an intermediary role might be, was a godsend. The U.S. government's acceptance of the 1955 Austrian State Treaty and of Austria's unilaterally declared policy of neutrality also indicated new U.S. recognition of the possible merits of neutrals' nonparticipation in the universal East-West struggle. As Gabriel points out, the "dualist" tendency of U.S. official thinking toward neutrality—"idealist" internationalism mixed illogically with "realist" traditionalism—still exists today.

Harto Hakovirta's somewhat more theoretical study, reflecting the experience of Finland since the mid-1950s as an openly self-described neutral, places the theme of neutrality within the context of "the master conflict" of the East-West struggle and does so in a balanced manner (p. 52). He emphasizes that in terms of its value premises and dependency relationships, European neutrality is "not neutral neutrality but Western neutrality" (p. 249). Nonetheless, for Finland particularly, geography is an unforgettable reality. Ireland's safe insular location—part of the reason why it has felt no need to join NATO and has been free to become a member of the European Community—is a counterpoint to Finland's equally asymmetrical situation. It is a strength of Hakovirta's book that he encompasses these "new outer limits" of contemporary European neutrality—including Malta, too—in an effort "to constitute and define neutrality as a category of states and policies" (p. vii) rather than to study them noncomparatively as a set of singular cases.

Perhaps his most important discovery—and Gabriel's as well—is that of the power inherent in neutral cooperation, particularly if in pursuit of "active peace policies" (p. 244). Gabriel interestingly discusses the proposal of a U.S. jurist, Philip C. Jessup, in the 1930s for a common and solidary front of neutral states—a neutral league—to supplement the League of Nations. Very possibly, this idea was part of what President Franklin D. Roosevelt had in mind when he spoke in October 1937 of a "quarantine" of aggressors. Hakovirta, focusing on a later period, highlights the role played today by neutral states in multilateral conference diplomacy, citing particularly Sweden's leading part in the multilateral disarmament discussions in Geneva and Finland's large contribution as "the main catalyst and host" (p. 224) of the 1975 Conference on Security and

Cooperation in Europe. As alliances weaken and as the world system becomes further depolarized, both authors predict, the banner of traditional, legally established neutrality—as distinct from neutralism, or nonalignment—may exert a stronger attraction.

ALAN K. HENRIKSON

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The Pacific Way: Regional Cooperation in the South Pacific. By Michael Haas. New York: Praeger, 1989. 183p. $42.95.

The Pacific Way is an effort to understand the process and status of regional cooperation among the island nations of the South Pacific. The study covers all intergovernmental cooperative arrangements—outside of the United Nations system—that have regular meetings and a membership of at least three sovereign countries from the South Pacific. The purpose of this book is to sketch the details of regional cooperation, address the prospects for a more coherent structure for cooperation in this region, and finally make a "refreshing contribution to theories of regional integration" based on a "new form of international interaction" (p. xxii). The strength of the book lies in the readable nature in which the author accomplishes the first of these. Less clear, however, is to what extent Haas's findings point to new and more promising theories of regional integration.

Haas's book will prove an invaluable resource for researchers interested in South Pacific cooperation. The waning control of the metropoles had led to an evolution in regional cooperative structures that have moved from those based on paternalism to those based on local control and initiative. Two intergovernmental organizations, the South Pacific Health Service, and the South Pacific Air Council, were based on the interests of the metropoles and have given way to regionally based organizations with similar functional concerns. And the South Pacific Commission, established by the Canberra Agreement of 1947, has given way to the South Pacific Forum, consisting of the regional "powers" (Australia and New Zealand), and the Forum Island Countries. This evolution in the basis of international cooperation is seen as a neutral and successful adaptation to the end of colonialism and the new independence of the smaller island nations.

The Pacific Way is largely devoted to presenting institutional analysis of a number of functionally specific intergovernmental organizations. Aside from the regional defense alliance, ANZUS (Australia-New Zealand-United States, which is based on U.S. power and excludes the smaller South Pacific countries), Haas is most concerned with organizations under local regional control. These address such concerns as the administration of justice, education, trade and shipping, and tourism. The South Pacific Forum, which brings together heads of state and deliberates on questions of policy as well as program proposals, is viewed as successful in furthering the well-being of the regions' people, though there is some evidence, in the form of the Melanesian Spearhead Group, of the tendency for the region to split along racial or national lines. The book also covers two other types of intergovernmental arrangements: entities headquartered in Hawaii and entities that have broken off from the United Nations. Since these tend not to be based on the cultural affinities of the region, they are not usually as successful as those with local roots. Overall, Haas does an admirable job of outlining the institutions that form the infrastructure of regional cooperation in the South Pacific.

The second purpose of this book is to argue that regional cooperation in the South Pacific is surprisingly advanced, as evidenced by the dense network of international organizations in such a sparsely settled part of the world. Altogether, some two dozen organizations have been formed among the countries of the South Pacific; and cooperation has brought tangible benefits in areas ranging from trade to telecommunications. But as the author notes, "the many activities of the South Pacific Commission and the proliferation of various regional organizations also outside the framework of the Forum have presented some difficulties" (p. 162), primarily problems of duplication of efforts in the context of very limited resources. Nonetheless, Haas argues that it is impossible to expect that a network of international organizations should grow and expand in ways that are perfectly rational; indeed, there may be advantages to a sort of inter-institutional system of "checks and balances to keep each organization on its toes" (p. 163). Whether this optimism is fully justified or not, it is a theme found consistently throughout the study.

Though interesting, exhaustive, and well researched, *The Pacific Way* does not live up to its promise to present a new theory of international integration. The author's contribution is to note that the diplomatic style of the peoples indigenous to the region, "shaped by a common interest of peaceful modes of state behavior," is one of consultation and compromise. It is based on a perceived "common identity" and a "compatible operational code in foreign policy" (p. 1). The clear implication is that outside interference is largely responsible for a slow start to indigenous cooperation. This diplomatic style, dubbed the *Pacific Way*, calls on South Pacific leaders to "unlearn Western modes of conflict resolution" (p. 9). The process of such a style involves the rotation of host country for meeting and conferences, discussion as the primary means of dispute resolution, and a sort of optimistic incrementalism in problem solving. The Pacific Way is also marked by informalism (frequently intergovernmental organizations in the region lack a charter or statement of purpose) and a desire for local solutions to local concerns.

Haas makes claims that turn realism on its head: he claims that in the "pan-Pacific spirit" collective priorities prevail over national self-interest and that South Pacific countries prefer cooperative strategies and see peace as the normal condition of international relations (p. 17). While these claims may be true in a loose sense, it is another problem to argue that they account for the number or success of intergovernmental organizations in the region. For one thing, the study makes no rigorous comparison between the "success" (however measured) of organizations that are more locally controlled and those in which outside influence is more significant. Secondly, the argument encounters the same problem as does much of the empirical "international cooperation" literature, that is, the failure to distinguish explanations based on *interests* from those based on some other explanation—in this instance culture or diplomatic style. Cultural affinities aside, we have every reason to believe that cooperation is an imperative of economic development—even of survival in this region of so few resources. The author himself suggests as much: "Unlike other regions of the world, the South Pacific would suffer greatly if regional cooperation were to disappear; the South Pacific Forum and other bodies are not luxuries, but instead are imperatives for countries with small populations almost totally lacking in lucrative cash crops" (p. 167). Why it is necessary to embroider an explicitly *cultural* explanation over this clear interest-based interpretation is unclear. Finally, the author perhaps exaggerates the uniqueness of South Pacific Cooperation by failing to distinguish the types of cooperative problems addressed by this litany of intergovernmental organizations. The most successful ones have more to do with problems of simple coordination and standardization (educational standardization); emphasize information sharing (the four organizations relating to the administration of justice); and/or are typically generously funded from the outside (shipping lines), making the distributive problem less pressing. The issue areas that have remained the most divisive (increasing tourism, regional airlines) are those where the distribution of benefits is contentious in the South Pacific, as they would be anywhere in the world. Though invoked religiously at the end of each section, it is difficult to assess what explanatory power ought to be given to the diplomatic style known as the Pacific way.

BETH A. SIMMONS

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Germany, America, Europe: Forty Years of German Foreign Policy. By Wolfram F. Hanrieder. New Haven: Yale University Press, 1989. 509p. $29.95.

The postwar history of Germany's relationship to the United States and Europe is an engrossing story of complicated international diplomacy and domestic party politics. The story, as told by Wolfram Hanrieder, is a masterpiece of political analysis. Through imaginative topical organization and sequencing of events, it lucidly portrays the political dynamics that led to the partition of both Europe and Germany and, equally importantly, to the general acceptance of this East-West division by the United States, the Soviet

Union, and their allies. It is also far and away the most judicious treatment to date of the 40-year-old controversy surrounding the question of German unity.

One of this book's great attractions is that while it represents a profound work of scholarship, embracing 117 pages of illuminating notes, it lacks academic pretense. Written in crisp and elegant prose, it commends itself to educated readers as well as specialists. Hanrieder's narrative never descends to the level of mindless patter about political personalities or tedious accounts of political decision making. He sticks to the historical record, always fixing our attention on the meaning of events as they unfold. He fills our plate with the details of military, economic, and monetary policy; but the details do not overwhelm us; they are blended together to achieve a marvelous synthesis of the German condition in the postwar era. The author's ability to connect events and clearly explain the development of policy from decade to decade is one of this book's impressive literary achievements.

Hanrieder's account is divided into four parts (and 13 chapters), dealing respectively with security and arms control policy, the partition of Germany and Europe, the political economy of the Federal Republic, and the meshing of foreign and domestic policy in Germany. The book's organization underscores its major theme: nuclear, territorial, and economic issues generated an interlocking network of alliances, rivalries, and policies that by the 1980s had resulted in the stabilization of a European state system predicated on the permanent division of Germany and Europe. The author is particularly adept in relating this development to the twists and turns of internal German politics as well as to the Federal Republic's evolving and changing relationship to France, the United States, and the Soviet Union.

Hanrieder's interpretation of postwar politics in central Europe is as plausible as it is faithful to the historical record. His principal contention is that the United States sought from the very beginning to contain both Germany and the Soviet Union: West Germany by its integration into a wider European community and the Soviet Union by the joining of U.S. nuclear power to West German rearmament and economic reconstruction. Within this framework—and later as a member of the North Atlantic Treaty Organization—West Germany sought to achieve her own independent goals of security, equal partnership within the Western alliance, and (most importantly) reunification. Yet, ironically, argues Hanrieder, these goals were seen as increasingly incompatible with U.S. geostrategic interests and arms control policies as well as with de Gaulle's attempt to reorient Franco-Soviet policy in the 1960s.

Contrary to the view of some historians, Hanrieder maintains that "necessity overwhelmed choice" in giving rise to the German problem. In his view, West Germany's political and economic recovery was absolutely dependent on her "close alignment with the West and the decision to rearm." In this sense, Adenauer's pro-Western policies "coincided with necessity." Where Adenauer went wrong—and where West German foreign policy failed—was in the conviction that reunification could be achieved from a posture of strength reinforced by U.S. nuclear power. This strategy, as Hanrieder reminds us, completely collapsed when the Soviet Union began to achieve nuclear parity with the United States, leading by 1960 to fundamental changes and tensions in West Germany's relationship to its closest allies, particularly the United States and France.

Hanrieder maps the course of these changes and tensions against the backdrop of transformations in the international balance of power, relating them in turn to political developments in Germany. Particularly fascinating is his account of West Germany's differences with the United States over nuclear and arms control policy and with France over economic and monetary policy. These divergent interests, which persisted into the 1980s, influenced the positions taken by the various political parties in West Germany on issues such as European integration and reunification. In the end, Hanrieder's argument can be captured in this incisive passage: "As always in the history of the Federal Republic, its inner responses in the 1980s corresponded to the outer circumstances of its diplomacy: polarized in the 1950s, refracted and convoluted in the 1960s, convergent in the 1970s, unfocused in the 1980s. The 'great compromise' that had emerged over the decades on the major outlines of German foreign policy continued to be enforced in the 1980s by the limits placed on German diplomacy" (p. 363).

It might be noted, finally, that this book went to press before Eastern Europe started to unravel. Hanrieder turns out to have been no more prescient in foretelling the collapse of the Berlin Wall and the Communist government of East Germany than any other scholar pretending to specialize in German affairs. From his vantage point in early 1989 German reunification remained, at best, a distant possibility. Within months it became a sudden probability as concerned nations scrambled to find ways of slowing the pace of German unity. The velocity of recent history has shaken many certainties. I am, however, certain that Hanrieder's book is mandatory reading for anyone wishing fully to understand the background and dialectics of the current controversy surrounding German reunification.

DONALD P. KOMMERS

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Rules, Norms, and Decisions: On the Conditions of Practical and Legal Reasoning in International Relations and Domestic Affairs. By Friedrich V. Kratochwil. Cambridge: Cambridge University Press, 1989. 317p. $49.50.

This book is about law, international law, language, and justification. It will not find favor with legal positivists, those who hold to a referential theory of meaning, those who decry as meaningless concepts that cannot be operationalized, or those who characterize the international system as anarchic. It will be applauded by those who recognize that language analysis can often dissolve perplexities, who champion "the importance of everyday language in understanding human action" (p. 43), and who find the distribution of capabilities less important than motivating beliefs and questions of legitimacy.

Much of the material in this work is not new. Kratochwil acknowledges 12 separate articles in which this material has previously appeared, but he has skillfully rewritten and organized them so that the book reads as a coherent whole. Too often, publishers allow authors merely to lump together previously published material between a new introduction and a conclusion with lack of revision obvious. In this book, the introduction outlines what follows. Each chapter begins with a preview of the coming attractions and ends with a summary of the arguments presented. The effort spent on this organization has produced a work that is easy to follow despite the twists and turns of the complex material that it presents. Although the work contains 49 pages of detailed notes, it would be improved by a separate bibliography.

Kratochwil's well-developed argument centers around the role of *norms* (which he uses interchangeably with *rules* and *principles)* in social action (p. 10). The important question he tries to answer is, Why do actors have to resort to norms? He begins by denying that norms are best understood as causes. They are guidance devices that simplify choices by reducing complexity. Human action in general is rule-governed: "It becomes understandable against the background of norms embodied in conventions and rules which give meaning to an action" (p. 11). Rules and norms influence choices through the reasoning process. Neither their logical necessity nor their aggregative weight is decisive in the debates over what is right: rather, the persuasive weight of claims is what convinces.

A short review cannot do justice to this literate and well-written work. In the first two-thirds of the book the argument ranges across an ordinary language analysis of the prescriptive status of rules, the development of the third party mode of reasoning as a nonviolent solution to social conflicts, and a thorough examination of the notion of rights as socially protected claims. These discussions are guided by a conceptual apparatus derived from speech act theory and a communicative action approach. The remainder of the work builds on these arguments and explores their implications for, and the applications to, both domestic and international legal systems. Kratochwil convincingly demonstrates that legal reasoning is a particular style of practical reasoning. By showing that the style of reasoning by judges in international courts is the same as that in domestic courts, he provides a positive answer to that hoary question of whether international law is really law.

It is the constitutive function of international law that is of real importance to Kratochwil. Following William Coplin, he claims that "the international legal order exists simply by virtue of its role in defining the game of international relations" (p. 251). Kratochwil maintains a special status for international law and rejects its reduction to mere political process. In our haste to make the study of international law acceptable to our colleagues in political science, we have too often operationalized away its special characteristics. Kratochwil's emphasis on the importance of the third party mode of reasoning reminds us that "international law is *applied* to problems by a special class of public officials" (p. 251; emphasis original).

This work is an important contribution to the debate about rules and norms, their role in social action, and their connection to law. It may help to reverse the prevailing view that international law is not a subject worthy of teaching in political science departments. The dismal decline in the study of international law is in part the result of our failure to demonstrate the relevance of its special modes of reasoning in human social action. Kratochwil has clarified my own thinking and will be required reading for my students.

J. STEVEN BRUNDAGE

University of Arizona

Inside the National Security Council: the True Story of the Making and Unmaking of Reagan's Foreign Policy. By Constantine C. Menges. New York: Simon & Schuster, 1988. 418p. $19.95.

When historians of the next century review U.S. foreign policy in the 1980s, they will try to make sense of the extraordinary attention given by the U.S. government, Congress, the media, and the public to the issue of Nicaragua. Year after year, throughout the Reagan presidency, Nicaragua took on an importance far beyond the country's small size and population, economic weight, or strategic relevance. A long-standing and recurrent problem for U.S. policy—how to respond to a revolutionary nationalist movement in one of the small countries of the Caribbean Basin border region—came close to dominating the foreign policy debate of the mid-1980s; it was in particular a permanently contentious issue in congressional-executive relations, as has been skillfully illuminated in Cynthia Arnson's *Crossroads.*

Constantine Menges, a political scientist and think tank policy analyst, contributes to the understanding of the Nicaragua policy debate in this account of his two years on the staff of the National Security Council (NSC). The prime significance of Menges's memoir is to illustrate that Nicaragua policy was not only an object of bitter public and congressional-executive struggle; it was a subject of intense debate within the Reagan administration, pitting career bureaucrats against political appointees, agency against agency, and clique against clique. Even members of the president's cabinet differed sharply with each other about the aims and appropriate instruments of U.S. policy toward Nicaragua, as well as about strategy and tactics. At all levels in the bureaucracy, these differences were compounded by ambitions and careerism, ideology and politics, personal styles and quirks, and by great variances in experience and skill.

The climate within the Reagan administration for policy making on Nicaragua was exceptionally destructive. Menges credibly portrays a White House staff where political appointees (like Menges) and career officials deeply mistrusted each other and where the Department of State was viewed as archenemy by the former and as a secret ally by the latter. Memoranda signed without authorization, meetings convened without notifying relevant parties, letters and documents intentionally misplaced or delayed, leaks and counterleaks, end runs and shortcuts, manipulation and deception of many sorts—all were apparently standard in the Reagan White House.

The argument Menges advances (not as persuasive as his depiction of the policy-making process) is that President Reagan had a clear policy of his own on Nicaragua, one that was faithfully implemented by CIA Director Casey, Defense Secretary Weinberger, Ambassador Jeane Kirkpatrick, and a handful of the president's other men and women among the various agencies. This allegedly clear presidential policy was systematically undermined and eroded, Menges asserts, by Secretary of State Schultz, NSC advisors McFarlane and Poindexter, the foreign service officer corps, and others within the administration who sought, in Menges's view, to manipulate the foreign policy process and substitute their own policy preferences for those of the president. On several different occasions, portrayed with unrelenting zeal (though with an admittedly incomplete perspective), Menges argues that the NSC, with himself as catalyst, rescued the president's policy from impending sabotage by those Menges calls the "accomodationists." Finally, in 1987, after Menges had been forced to resign, Speaker of the House Jim Wright and the State Department finally conspired to scuttle the Reagan policy and to substitute U.S. support for the peace plan advanced by Costa Rica's Oscar Arias. What many observers now see as the rescue from disaster of U.S. policy toward Nicaragua, responsible for the ultimate electoral process and the ouster of the Sandinistas, Menges himself sees as the final betrayal of Ronald Reagan's approach.

The Achilles tendency of this account is Menges's urge to equate his own view of Nicaragua squarely with that of President Reagan. Himself the author of Reagan's affirmation, "A Communist Central America poses the grave risk that one hundred million people from Panama to our open southern border will come under the control of pro-Soviet dictatorships," Menges extrapolates from the president's endorsement of this view a settled U.S. government determination not to negotiate with the Sandinistas except about their surrender; others in the administration believed that the United States could be spared the specter of one hundred million communists on our border without necessarily overthrowing the Sandinista regime. Far from making his own position clear, the president actually left both options open; and U.S. policy thus came to be shaped by the permanent struggle Menges chronicles.

Menges argues that a future president should bring in his own policy team, a "politically appointed foreign policy subcabinet" of about three hundred individuals with shared values to staff the foreign policy bureaucracy much deeper into their midlevels. Many readers, however, will draw exactly the opposite conclusions from this account about the proper role of political appointees.

ABRAHAM F. LOWENTHAL

University of Southern California

Handbook of War Studies. Edited by Manus I. Midlarsky. Boston: Unwin Hyman, 1989. 372p. $49.95.

Given the vast array of topics in international conflict and the increasing technical expertise required to understand many topics, it is not surprising that from time to time topical summaries emerge. These stocktaking efforts have evolved from little more than edited texts of articles *au courant* in the literature to well-developed theoretical and empirical evaluations of research programs and agendas, for instance, Gurr's *Handbook of Political Conflict,* (1980), Hoole and Zinnes's *Quantitative International Politics* (1976), and Hopmann, Singer, and Zinnes's *Cumulation in International Relations* (1981). The most recent effort in this regard is Midlarsky's *Handbook of War Studies.* It contains the kind of detailed consideration of war that is usually only available in book or monograph length. The topics covered in the volume include as explanations arms races, power cycles, geographic diffusion, lateral pressure, and public opinion, among others. The added advantage of having all topics treated in a single volume cannot be overlooked. In addition to the topics covered and the depth of their treatment, the editor has relied on one or more of the individuals who were, or continue to be, central to the evolution of theory in that area to write the chapter, thereby contributing to the authoritative flavor of the discussion. The volume contains some technical material; but the vast majority of the text is expository, with graphs, charts, tables, and some equations used to elucidate the prose.

The *Handbook of War Studies* seeks thoroughly to evaluate the predominant theories in the scientific study of war. The book is organized into three theoretical sections covering systemic, dyadic, and state-centered explanations of war. Under each heading, individual chapters address various explanations for war. Some chapters are updated summaries from longer manuscript-length works of the authors, for example, "Public Opinion and National Security" by Russett and Graham; "The Diffusion of International Conflict" by Most, Starr, and Siverson; and "The Power Transition" by Kugler and Organski. Several chapters are more concerned with self-evaluation or integration of conflicting empirical results: "The Diversionary Theory of War" by Jack Levy and "The Contribution of Expected Utility Theory to the Study of International Conflict" by Bruce Bueno de Mesquita fall into the former category, while "Long Cycles and Global War" by Modelski and Thompson and "Arms Races, the Conflict Spiral, and the Onset of War" by Siverson and Diehl fall into the latter. Each chapter attempts to outline both major research questions and flaws and problems in theory and methods—generally, to establish the state of the art in that area and identify what issues remain unresolved.

Two contributions stand out because they make a particular effort to reconsider the basic issues of concept and theory in the study of war: Singer's lucid "System, Structure, Decision Processes and the Incidence of International War" and Choucri and North's "Lateral Pressure in International Relations," which includes a case study of Japan. Two contributions are particularly useful because they summarize a highly technical area in a way that even a non-specialist in the area can easily follow. Siverson and Diehl's discussion of the *outcomes* of arms races and Intrilligator and Brito's chapter on arms race *processes* succeed in organizing the considerable amount of cumulative results in arms races without overwhelming the reader in equations.

The *Handbook's* overall contribution, however, is marred in two ways. First, the analytic distinctions between system, dyad, and state-centered theories appear to be more of a contrivance around which to organize book chapters than a well-developed framework to aid cumulation. The authors seem relatively unimpeded by their section headings and feel no need to justify why their theory should be viewed as "dyadic" rather than "systemic." For example, why is a Poisson process of the diffusion of international conflict a structural- rather than a dyadic-based theory; how is expected utility a dyadic rather than an individual-level theory? Despite the numerous references to Lakatos, the chapter authors fail to integrate his standards faithfully into their evaluations. A more thoughtful consideration of how the variety of "war studies" actually fit together as research programs and the extent to which they contribute to "additive" or "integrative" cumulation (to borrow some terminology from Zinnes) would have greatly increased the value of this book to the study of war.

Second, certain topics need more integration if only of the "additive" variety. In particular, Doran's chapter, "Power Cycle Theory of System Structure" overlaps with Kugler and Organski's "Power Transition" and to a lesser extent with Modelski and Thompson's "Long Cycles and Global War." Systemic and cylical explanations of war overlap in this handbook but are not sufficiently synthesized. Different jargon is used to express the same ideas. The result is an ungainly component to an otherwise well edited volume. This flaw may say more about the state of the art in cycles research than it does about the editor's efforts to integrate this research into the handbook; other chapters are better integrated, relying on common jargon and citing similar work. The two chapters on arms races, for example, are so deftly written that the reader easily perceives the range of issues under this topic heading, how they fit together, and the cumulation and the theoretical rigor in the arms race literature—all which serve to showcase this field.

True to its genre, this volume is not concerned with a single theory but instead commits itself in the introductory pages to respect the variety of theories of war. This is not to say, however, that the book lets "a thousand flowers bloom." On the contrary, it is strict in its operational definition of theory, harboring scientistic bias and excluding controversial approaches like world systems analysis, formal theories such as game theory, methodologically centered theories such as artificial intelligence and traditional theories such as balance of power. Some of these omissions can be explained by the book's focus on only three levels of analysis, others by the book's focus on war. The focus on system, dyad, and state to the exclusion of other levels of analysis retains arbitrary lines between levels of analysis. The focus on war undercuts generalization of these theories to conflict processes besides war. The restricted coverage both in terms of *explanans* and *explanandum* precludes the kind of integrative cumulation the field needs, cumulation that synthesizes a multiplicity of explanations extending beyond war to other forms of international conflict. The current focus limits the utility of the handbook to analysts and practitioners who could be guided in their own research programs and decision making by understanding how these topical explanations of war contribute to each other and to other forms of international conflict.

Despite its flaws, the *Handbook of War Studies* succeeds as more than another stocktaking exercise because most of the chapters have utilized the medium to reevaluate the theory and empirical evidence in a particular area germane to the study of war. The effort is narrow, and many important questions of cumulation in the study of war remain unresolved. Yet the authors genuinely attempt to resolve contradictions within the confines of their expertise, reconsider evidence, and offer new evidence as well. What the book lacks in integration it makes up for in its depth, detail, and evidence.

LOIS W. SAYRS

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The ANZUS Crisis, Nuclear Visiting, and Deterrence. By Michael C. Pugh. Cambridge: Cambridge University Press, 1989. 285p. $44.50.

In early 1985, New Zealand's Labor party prime minister, David Lange, refused a port visit to a U.S. ship that could have been carrying nuclear weapons. Pugh writes in the preface that he wishes to provide "a balanced interpretation of the ANZUS (Australia-New Zealand-United States) crisis in the context of changing security concerns in the South-West Pacific." He succeeds only in producing a thorough assessment of changing security concerns.

His effort follows four broad themes: (1) problems in alliance management, (2) globalism and regionalism in international relations, (3) "nuclearism" versus "antinuclearism," and (4) the role of small powers in today's international environment. These are directed to linking "internal and external aspects of state behavior and throw[ing] light on the stress in international relations between parochial perspectives and the conception of a seamless nuclear web" (p. 16).

Indeed, if blame can be assigned to causing the cooling of a close alliance, Pugh places it on a blind adherence to the "seamless nuclear web." This was manifested in the U.S. policy of *neither confirm nor deny* (NCND), or not revealing the presence of nuclear weapons on ships making port calls to foreign nations. The United States believed this policy essential to its task of defending the West through nuclear deterrence. Therefore, the United States felt, allies had to accept obligations that entailed allowing nuclear visits. New Zealand felt it should be able to disallow such visits; and in absence of a U.S. statement that a ship was *not* carrying nuclear weapons (because of NCND), it had to deny such visits.

Thus came the split in ANZUS—but an avoidable one, Pugh argues. The "seamless web" belief system instilled in the Reagan administration he sees as unable to recognize New Zealand's changed needs and growing nationalism. The latter no longer saw the relevance of being in an alliance with a nuclear emphasis and wished to make its participation nonnuclear. Pugh views the larger lesson as being that smaller allies will continue to prefer qualified alignment (although not neutralism or nonalignment) as global relationships change—especially current trends between the superpowers.

However, it is difficult to justify this interpretation as "balanced." For example, Pugh misses few opportunities to label the U.S. administration as "ideologues," "right-wing," and in the best display of rhetoric, bestows "Reaganite apologist" on an author who published a pamphlet under the National Defense University imprimatur. Such remarks might be less noticeable had similar rhetoric been used in description of the Lange government. Although Pugh interprets New Zealand's actions in nonideological terms, this is hard to justify. The left wing of the party did make the government adopt a policy associated with the Left; and Lange stated many times that New Zealand initiated the ban due to dissatisfaction over arms control progress, an act a small nation could do to promote peace. Pugh depicts the Lange government as resisting the left wing of the party.

To an extent, this is true; in fact, the Lange government and its successor have been more free-market-oriented than the previous National party (considered conservative). This is what Pugh uses to declare that "ideology had limited relevance" in the decision to declare a ban. But he draws this conclusion only because he considers antinuclearism as nothing truly radical; the term *left-wing* is never applied to these views. Maybe Pugh does not consider them so because much of the New Zealand population subscribes to them.

Yet treatment of other issues makes it difficult to maintain that the analysis is not destined to turn out a certain way. He presents several conspiracy theories, almost universally doubted, involving CIA intrigue in the South Pacific (pp. 133–34) yet lightly passes off suspected communist influence in the region's peace movements (pp. 106–7). He reminds us that Tonga's critique of the Rarotonga Treaty establishing a nuclear-free zone in the South Pacific is by a "major recipient of U.S. aid" (p. 167); yet only paragraphs before he dismisses the influence of Libyan aid to Vanuatu, who criticized the treaty as being too lenient. Opposition to the ban in Congress is said to be a function of conservative ideology and economics (pp. 136–38), but this conveniently omits that one of the biggest critics of Lange was Stephen Solarz, known to be very sympathetic to leftist causes.

These considerations lead Pugh to miss the mark in explaining why the breach occurred. Journalist Stuart McMillan's arguments in *Neither Confirm nor Deny* (1987) are more compelling, identifying the misunderstanding as a product of differences in geography, culture, and society and in the structures and behaviors of political institutions. Pugh does a fine job in presenting details about how ANZUS came about, how the signatories' goals have changed, and the role of liability and safety for visits and identifies the larger question of the value of deterrence versus the democratic goals it guards. Unlike previous authors, he tries to put the question into a larger theoretical focus. To this end, he gives good insight into what led up to and followed the break, but his analysis of the break itself is unsatisfactory.

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U.S. Unilateral Arms Control Initiatives: When Do They Work? By William Rose. Westport, CT: Greenwood, 1988. 199p. $39.95.

American Interests, American Purpose: Moral Reasoning and U.S. Foreign Policy. By George Weigel. New York: Praeger, 1989. Center for Strategic and International Studies, Papers no. 139. 96p. $34.95 cloth, $11.95 library binding.

The Nuclear Seduction: Why the Arms Race Doesn't Matter—and What Does. By William A. Schwartz and Charles Derber. Berkeley: University of California Press, 1990. 294p. $25.00.

Nuclear Deterrence and Moral Restraint. Edited by Henry Shue. New York: Cambridge University Press, 1989. 435p. $49.50 cloth, $16.95 paper.

These four studies take the measure of current thinking about the morality and prudence of nuclear deterrence. Rose's monograph defines unilateral initiatives as "actions of military restraint undertaken by a government . . . the publicly stated purpose [of which] is to induce [some other country] to change its military policy" (p. 4). He identifies 10 "obvious" and 13 "hypothetical" variables that affect the success of unilateral initiatives and deploys this ensemble in a comparative analysis of five historical cases: the U.S. decisions not to orbit nuclear weapons (1962), to forego atmospheric tests (1963), to scrap medium-range bombers (1964–65), to build only a thin ABM (1967–68), and to forego neutron bombs (1978). Rose concludes that initiatives succeed best in situations of trust, communicative clarity, manifest reciprocity, mild tension, and internal stability: they work best when they are least needed (p. 136). Despite this irony, they are still worth taking.

Rose's 5 cases multiplied by 23 variables make for difficult reading. Nevertheless, since he presents 13 variables as "hypotheses" to be "tested" by cases, his conclusions seem evidence-shy. Testing the hypothesis *The lower state A perceives the level of international tension, the greater the prospect that A's unilateral initiative will succeed* (p. 18) requires 2 cases alike except for the variable of tension; thus, "testing" 13 hypotheses would require at least 26 cases, not 5. Rose's structural background, which invokes Axelrod's studies of cooperation, provides little help, since *tit for tat* in Axelrod's studies succeeds only in multipolar tournaments, and these are poor models of bipolar superpower confrontation. Indeed, many of Rose's cases fail to qualify as genuine cooperative gestures, since the act of restraint would still be reasonable even if it were not reciprocated. Despite these weaknesses, Rose's study shows nicely that Soviet responses to U.S. initiatives have been more rational than rabid.

Weigel's study is an extended footnote to his much longer *Tranquillitas ordinis* (Oxford, 1987), which argued, against pacifist heretics, that peace requires order and order requires strength. The target of *Tranquillitas* was the quasi-pacifism of the U.S. Catholic bishops; the target of *American Interests* is the full-fledged nuclear pacifism of Boyle, Finnis, and Grisez's *Nuclear Deterrence, Morality, and Realism* (1987). Weigel thinks that their disarmament requirements are so ludicrous and dangerous that he hardly bothers to refute their arguments. Weigel censures them because their proposals for U.S. nuclear disarmament raise the risk of nuclear war—between the Soviet Union and China (p. 44)! But his own recommendations for "support for democratic movements" are perhaps as likely to raise the risk of nuclear war should the democratic movements be in the Ukraine. One can sympathize with Weigel's impatience with the utopian clarity of philosophical arguments and yet disagree with his view that ambiguous problems demand ambiguous principles. If that were true, the only ichthyologists would be fish.

The Nuclear Seduction seeks to break the mold of traditional thinking on nuclear matters. Like Bernard Brodie at the end and unlike Albert Wohlstetter even now, the six authors contend that the balance of terror is not at all delicate. All the major nuclear powers have secure second-strike capacity and no prospect of losing it. That a nation has nuclear weapons matters very much. Which nuclear weapons it has matters very little. As Richard Garwin once remarked, when a nation possesses twenty thousand nuclear warheads, the marginal value of a few warheads is nil. On this view, longstanding debates about targeting policies, weapons systems, and force sizes are distractions from the serious nuclear issues. (Of course, nuclear disarmament would make a difference, but it won't happen.)

The serious nuclear issues are the risks of nuclear war that superpower political leaders are inclined to run. That leaders have been running these risks for 45 years requires consulting the historical record; so the authors provide a fresh history of the nuclear age, exploiting the new historiography of David Alan Rosenberg and other specialists, so far largely confined to professional journals. The weaving of all this new research into a comprehensive narrative is a substantial achievement; and even the cognoscenti may be unsettled by the year-by-year accounts of what Eisenhower really planned in the Taiwan Straits crisis, what Castro really did in October 1962, and so forth. Given the outcomes, even small risks are morally significant, and the authors find nothing in U.S. postwar policy that morally compensates for running these risks. The same goes for the other nuclear powers.

One can admire the historical narrative and deplore nuclear risk taking without fully agreeing that weapons choices hardly matter. For one thing, weapons choices are entangled with military planning; and, as Morton Halperin (*Nuclear Fallacy,* [1987]) and others argue, U.S. military planning is dangerously dependent on nuclear options. Second, weapons choices have symbolic weights that affect the risk of nuclear war. A cool and rational person might agree with McNamara that placing nuclear weapons in Cuba does not affect the strategic balance, but the inevitable public reaction to the symbolism of nuclear weapons in Cuba made nuclear war more likely. So also for SS-20s, Pershing IIs, and so forth; and so also—in the other direction—for the INF treaty, which the authors dismiss as trivial.

More importantly, one can believe that the nuclear balance is quite stable and still worry about what should be done if deterrence fails. Here weapons choices can make a great difference, and these choices form the main subject of debate in *Nuclear Deterrence and Moral Restraint.* Unlike *The Nuclear Seduction,* Shue's volume's nine authors do not profess a common view, though their essays share a common excellence that speaks of indefatigable editing. Its main virtue is its powerful presentations of contrasting options. Leon Sloss defends the present countervailing strategy; Henry Shue finds it incoherent. David Lewis and Robert Foelber argue for *finite* *counterforce;* George Quester and Harald Feiveson speak for *finite countervalue.* C. A. J. Coady condemns all of the preceding as immoral, and Steven Lee finds that SDI will not resolve these moral problems. Those who accept Coady's critique will be intrigued with the detailed arguments for a nonnuclear deterrent in Europe presented by Lutz Unterseher. In these essays *finite* seems to combine what others have called *basic deterrence* (not extended to allies)—and *minimal deterrence* (the smallest possible arsenal that will do the job).

Perhaps the most intriguing essay in Shue's volume is Lewis's "Finite Counterforce." The moral critics of nuclear deterrence complain that counterforce strategies are immoral because they are unstable and that countervalue strategies are immoral because they require murderous intent. Lewis argues that a small counterforce deployment would neither be unstable (because it is manifestly unsuitable for first strikes) nor involve murderous intent (since U.S. weapons would be aimed at enemy weapons, not people). Hence, "Buy like a *MAD*man; use like a *NUT*" (p. 73).

On this view, finite counterforce does not involve moral planning for immoral deeds or rational planning of irrational acts. Its discriminate second strikes serve the moral and prudent aim of damage limitation. Such "damage limitation" will kill millions of people, but it will save millions of U.S. lives. This the president is morally obliged to do. As it happens, finite counterforce seems not far from what we currently practice: the Pentagon develops counterforce systems, and the Congress funds only finite deployment. It is a rare philosopher who finds Main Street almost all right.

Lewis agrees that the risk of nuclear war with a finite countervalue deterrent is the same as that of his counterforce deterrent. So the difference between these two systems will emerge only after a first strike, when the forces of both sides are reduced. At this point, it would seem that the instabilities normally associated with counterforce systems would reemerge for the finite counterforce deterrent and that the president would have no choice but to "preempt," that is, launch a second strike. Finite counterforce will not destabilize in peace; but it will destabilize the second phase of a nuclear war, and the war will end only when arsenals are exhausted. Doing nothing at all might be far better.

Furthermore, an affection for counterforce fuels a qualitative arms race. McNamara discovered this shortly after announcing *flexible response* and ate his words. Feiveson's proposal for a 90% cut in strategic arsenals does not suffer from this bureaucratic instability, but the moral issues of countervalue targeting remain. Quester has no qualms about them: since the finite countervalue system is best, it must be moral; and if our principles deny this, we should adjust our principles. Those offended by Quester's "necessary moral hypocrisy" are left with what Coady calls "the problem of extrication." The message of the philosophers is that the simple immorality of getting into the nuclear arms race makes for no morally simple way out.

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War: Ends and Means. By Paul Seabury and Angelo Codevilla. New York: Basic Books, 1989. 306p. $19.95.

Perhaps somewhat accidentally—indeed paradoxically—this work is timely because it runs directly against the grain of the now euphoric 1990s zeitgeist. One might not entirely agree with the opening sentence of the dust jacket summary, however, which states that "the study of war has fallen out of favor, shunned by our schools and ignored by a foreign-policy elite with little experience of military service." To the contrary, national security and war studies flourished during the 1980s. Only in the early 1990s do they seem about to fall out of favor as Soviet communism implodes, the Cold War fades, and the endemic violence of the Third World is increasingly ignored at the expense of the new big-league ball game—the economic-technological competition within the U.S.-East Asia-EEC triangle.

How did this paradoxical timeliness occur, then? As I write, there just appeared two essays: "An Idea Whose Time Is Fading" by Wade Green (*Time* 28 May 1990, p. 90), arguing that national security itself is now to be replaced by more multilateral and globally communal notions such as common security, mutual security, global security, and international security. "For New Foreign Policy" an op-ed piece by Charles William Maynes, editor of *Foreign Policy* (*New York Times*, 23 May 1990), calling for the wholesale replacement of the current foreign policy elite in Washington (election results, presumably, notwithstanding), which then would bring to the fore a new corps of *multilateralists*—"people who understand international financial institutions may be more valuable than people who know the names of Salvadoran guerrilla leaders." In short, exit "high politics" and enter, stage left, the brave new world of "low politics." Some commentators caught up in this euphoria even went so far as to predict that the very institution of war was about to depart the stage of history just as slavery has done during the past century or so.

The Seabury and Codevilla book, presumably written (if not published) before the astounding events of 1989-90, exhibits an altogether different and opposite mind-set. One assumes that the authors would scarcely change a word if they were only now meeting a publication deadline. Hence, in the opening pages: "Our book is inspired by the thought that the potential destructiveness of modern war has obviated neither the lessons of past wars, their political origins and outcomes, the general relationships between war and peace, nor the particular relationship among war, strategy and politics" (p. 6).

It is not easy to summarize or briefly characterize this book. Perhaps one could call it a lengthy, general essay on war in all of its ramifications, sort of a *tour d'horizon* that Edward Luttwak states in a dust jacket blurb is "an astonishing achievement of compression." It covers a lot of ground and in a way is an updated, truncated follow-on to Quincy Wright's massive *Study of War*—the authors' own criticism of Wright's proclivity for seeking "generic causes" of war notwithstanding.

One is aware immediately of Paul Seabury's inimitable writing style—erudite, worldly, witty, slightly ironic, chock-full of historical illustrations, and very readable. Not surprisingly, the book is written from a recognizably conservative perspective—hence, among other things, the insistence on the endemic and durable nature of warfare and its basic seriousness, in contrast to the emerging mood of the 1990s. Otherwise, the work also is suffused with strong suspicions about behavioral conflict studies and peace research; it is insistent, too, on the complexity of a subject not easily reduced to generalizations, theories, or easy conclusions of any sort.

The book is divided into three main parts: "How Wars Start," "How Wars Are Fought," and "How Wars End": The causes, the conduct, and the termination of wars pretty much subsume everything falling under the study of war.

The section on the causes of war evinces skepticism both about generic causes (causal, empirical theories) and the attribution of accidental causes ("Political enmity is anything but accidental" [p. 46]). Rather, the authors prefer voluntarist theories, "stressing that most if not all wars are deliberately caused and waged over important causes . . . wars are intentional and serious undertakings, not accidents or frivolous adventures" (p. 60). There is an insistence that wars have always been very important determinants of the course of history and that the difference between winning and losing, like it or not, is a real one. The sharp Western, Christian distinction between states of war and peace is contrasted with the different Islamic and Chinese traditions.

The section on the conduct of war is quite comprehensive. Not surprisingly, given the previously noted methodological predilections, the first chapter is entitled, "The Fog of Wars." The role of leadership, the importance of national morale, and the relationship between ends and means (i.e., strategy) are also stressed. "Material Conditions of Battle" contains a welcome up-front emphasis on military geography, perhaps the most neglected area of study in modern warfare. The excellent discussions of intelligence work and space-based strategic defense reflect the authors' own recent endeavors in Washington. (Codevilla also has written extensively about SDI).

Perhaps the best part of this book is toward the end of "How Wars End." It could stand on its own as a signal contribution to the growing literature on war termination. Its strength lies in its treatment of negotiated "happy endings" (compromises) as only one of many possible outcomes of war. Some of the other possibilities are under the almost self-explanatory subheadings: "Capitulation," "The Peace of the Dead," "The Peace of the Prison," and "The Peace of Cultural Conquest." Additionally, one is reminded throughout the book of some of the horrors that are rarely put onto the daily press's front pages, namely, the remarkably sanguinary recent and ongoing wars in such disparate locales as Peru, Mozambique, Eritrea, Southern Sudan, East Timor, Sri Lanka, Lebanon, and others, not to mention the for-now-silent "Gulf War." One also is reminded that in earlier periods of history, genocidal massacres were routine and expected.

Whether the current prophets of "endism," as applied to the future of warfare, will prove to be prescient remains to be seen. The jury will presumably be out for quite some time on that one. For a more timeless view, this work on war is a good place to start.

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UNIFIL: International Peacekeeping in Lebanon, 1978–1988. By Bjørn Skogmo. Boulder: Lynne Rienner, 1989. 277p. $28.50.

The cragged hills and lush wadis of South Lebanon have been the backdrop for a complex, controversial, and frustrating episode in United Nations peacekeeping. Since 1978, when the United Nations Interim Force in Lebanon (UNIFIL) was created with the heavy urging of United States, a polyglot collection of soldiers from Fiji, Nepal, and a dozen other countries have attempted to keep the peace amidst a host of regional and intra-Lebanese conflicts. Norwegian soldiers have been a part of the force since its inception; so it is not surprising that an experienced diplomat, who served for five years as a member of the Norwegian mission to the United Nations, decided to write a comprehensive case study about UNIFIL.

Bjørn Skogmo writes with the authority of a participant-observer about the diplomatic skirmishes, ambushes, and battles that have enmeshed UNIFIL. His purpose is to identify and explain the main constraints on the management of a peacekeeping operation. While Skogmo's book is a generally reliable descriptive account of UNIFIL, it is a uniquely valuable and judicious account of how the UN Security Council and Secretariat oversee and direct peacekeeping operations. A motif of the Security Council is to pass the buck to the secretary-general whenever it fails collectively to solve a problem, and the tendency for the permanent five members to retreat from responsibility is marked. The United States, the impetus of UNIFIL's creation, girded its formulaic statements of support with clamoring passivity, especially during the Reagan era. In his customary subdued prose, Skogmo notes that it would be "highly desirable" for the permanent members to play a more active and forceful role; he means especially the United States.

But even with a significant increase in support from the Security Council, UNIFIL is incapable of fulfilling its mandate under UN Security Council Resolution 425, namely, to confirm the withdrawal of Israeli forces, restore international peace and security, and assist the Lebanese governments in restoring its effective authority. But Skogmo emphasizes that the force does play several key roles: it is a crucial buffer force, a hard-to-replace component in a delicate concatenation of forces; it delivers and facilitates the delivery of humanitarian assistance; and by its very presence it complicates the effort of any regional player to further erode Lebanese sovereignty. Equally important is the fact that UNIFIL serves as an anchor for moderation, particularly in the Lebanese Shiì Muslim community, where, on the popular level, the force enjoys widespread, if not unanimous support.

Most peacekeeping operations involve the fairly routine deployment of an interpositional force to separate belligerents who have accepted its deployment, often to verify a disengagement or cease-fire agreement. In contrast, UNIFIL was dropped in the middle of several internecine conflicts with, at best, only grudging consent from the combatants. Although the weak Lebanese government welcomed the force, UNIFIL was a fait accompli that both Israel and the Palestine Liberation Organization—albeit for different reasons—were hard-pressed to reject. Faulty and incomplete consultations at inception left a legacy of mistrust and hostility. As a result, UNIFIL has often appeared on the Security Council's agenda precisely because its soldiers are not universally viewed as part of a solution.

UNIFIL was hastily deployed (too hastily Skogmo hints), even considering that the purpose of the force was to oversee the withdrawal of an invading army. Ad hoc decision making, especially in the heat of an international crisis, has its obvious virtues; but too often spur-of-the-moment decisions prove to have been made with inadequate considerations of their consequences. More than 12 years following its deployment, UNIFIL continues to be haunted by faulty decisions made with too little guidance from the Secretariat and by people who were thrust, with little preparation, into fast-moving and very complex situations. Skogmo's sensible, if modest prescription is the establishment of a skeletal military staff in New York, an infrastructure "able to act quickly and professionally in the case of new assignments" (p. 173). Others, including myself, have come to the same conclusion.

UNIFIL: International Peacekeeping in Lebanon is an important addition to the literature on international peacekeeping. Moreover, this evenhanded and meticulously researched case study is written in sturdy, jargon-free prose that nonspecialists will find accessible. It almost goes without saying that the book will be therapeutic reading for those contemplating ambitious exercises in UN peacekeeping.

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The Domestic Analogy and World Order Proposals. By Hidemi Suganami. New York: Cambridge University Press, 1989. 238p. $44.50.

A fundamental debate in international relations theory revolves around the appropriate approach to global reform. Perhaps the most significant split among reform-minded thinkers is whether to fashion world order solutions on the basis of transferring norms, procedures, and institutions from domestic life or to work within the states system as now structured to maximize its distinctive ordering capabilities. The outer limit of the former approach is a full-fledged world state; the outer limit of the latter is a humane, prosperous, and balanced international society composed of serene and satisfied sovereign states. Of course, both ends of the spectrum tend to be ideal types, with the locus of serious assessment and prescription falling in between.

Hidemi Suganami, a Japanese scholar long resident in Britain, has brilliantly and imaginatively explored this dialogue among global reform theorists in *The Domestic Analogy and World Order Proposals.* There is no comparable book organized around analytic and historic themes with nearly Suganami's sweep, though perhaps Wesley T. Worsley comes closest in his survey of supranationalist thinking since World War II, *Alternatives to Anarchy* (1988). The argument is elegantly conceptualized around "the domestic analogy," thereby differentiating thinkers who rely on the domestic analogy to evolve their ideas about how to reform international relations and those who don't. The split is less obvious than might appear. For instance, as Suganami shows, two theorists may both be realists while one accepts and the other rejects the domestic analogy. Thus, Hans Morgenthau, the patron saint of contemporary realism, is a firm adherent of the domestic analogy when it comes to an image of global reform, which he predicates on a world governmental idea, made reliable by adequate enforcement machinery. Despite such an outlook, Morgenthau is a dire pessimist about human destiny, since he regards that which is necessary to be unattainable. In contrast, Hedley Bull rejects the domestic analogy when he comes to consider global reform, conceiving of domestic analogy proponents as diversionary and fanciful, setting forth his own view that states generally value their reputation for keeping agreements and restrained in their recourse to force by simple realist virtues like "prudence." Oddly enough, Bull is a guarded optimist in tone, affirming the sufficiency through history of these ordering instruments embedded in the very structure of anarchic international relations. Suganami explores these antagonistic differences over the course of the last two centuries, in relation to leading thinkers in the nineteenth and twentieth centuries, as well as in critical historical settings—such as in response to the two Cold Wars. As might be expected, recourse to the domestic analogy has been greatest in thought and action in the aftermath of breakdowns of international order. Suganami convincingly explains how the domestic analogy underlies virtually every project to create international organizations for peace and security (League of Nations, United Nations) and to subject international uses of force to the scrutiny of law ("aggression"). And he also shows the opposite cycle of perception and proposal, as in relation to the League and the United Nations. Their "failures" gave rise to skepticism about the domestic analogy, inducing scholars and leaders to base their hopes for world order on the stabilizing opportunities (e.g., deterrence, bipolarity, hegemonic stability) implicit in a world of conflictual states without reliance on prescriptive and institutional reordering. After a brief romance with the United Nations as the basis of peace and security after 1945, reliance was placed on "balance," "containment," and "rationality" to sustain international order at tolerable levels during the Cold War. The pedagogic value of this emphasis on the domestic analogy is to focus attention on what seems to work in international life and under what political conditions.

Suganami provides a clearly demarcated road map through the thickets of reformist thought. He has a gift for clear taxonomy, dividing proposals into six historical periods, with five sets of basic positions that capture the main variations in substantive content. He also brings a sophisticated critical sense to bear, identifying where influence may be exerted under a given set of circumstances. For instance, he asserts that the proposals of the World Order Models Project "may turn out to be beneficial in their ability to direct grassroots movements for world order, but . . . too ambitious to be taken seriously by governments for the foreseeable future" (p. 196).

In sum, I find this to be an excellent book, analytically rigorous, exceedingly well informed, exhibiting a capacity for careful reading and exposition of complicated ideas, while somehow keeping a conceptual eye on the central issues of concern. It deserves the widest possible audience, if only to open mainstream international relations theorists to a more sensitive appreciation of their own often-hidden preoccupations with making the world a better place.

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Conflict and Consensus: The Struggle between Congress and the President over Foreign Policymaking. By Gerald Felix Warburg. New York: Harper & Row, 1989. 356p. $29.95.

The struggle between Congress and the president over foreign policy making is as timeless as the republic. Its form and content have varied from one historical setting to the next, and one institution sometimes appears dominant only to have the other become so later; but the struggle endures. Warburg usefully explores its character and content in light of the institutional reforms stimulated by the Vietnam War in the 1970s, with particular emphasis given to the issues dominant during the 1980s.

Conflict and Consensus is informed by the fact that the legislative–executive struggle displays a distinctively cyclical character "in which first one branch, then another, has gained ascendancy in shaping U.S. foreign policy" (p. 53). Still, Warburg urges that "since the end of the Nixon administration, this enduring battle has developed new characteristics. In no previous era of congressional ascendancy has the United States borne the burdens of world leadership. And in no previous era of presidential counterreformation has the White House confronted such a formidable array of procedural weapons at the legislature's disposal" (p. 309).

If the 1970s was a decade of revolution in legislative–executive relations, the 1980s was a decade of counterreformation that "curbed many legislative prerogatives that were central to the congressional revolution of the previous decade" (p. xxiv). *Conflict and Consensus* is concerned with the interaction of the two. Its perspective is played out in Warburg's conclusion that "the Reagan administration challenged the powers of Congress across the board" (p. xxiv), while policy makers at both ends of Pennsylvania Avenue preferred "parliamentary gimmickry and legislative 'ad hocery' " in dealing with the crises at hand to a systematic consideration of "the more general underlying questions" that "muddy the law and divide the institutions of Congress and the presidency" (p. xxv). Congress nonetheless has not reverted to the acquiescent role it played during the Cold War years. Advocates of the reforms of the 1970s saw in them a means to "right the balance" between Congress and the executive in foreign policy making. The experience of the Reagan years demonstrates that the president remains preeminent in foreign policy and that Congress is less assertive than in the previous decade, though not pliant. The Senate's role in the ABM treaty reinterpretation dispute and the House's role on the issue of Contra aid, among others, are illustrative.

Conflict and Consensus is divided into three parts. The first examines briefly the historical record of legislative-executive competition in foreign policy; the second the procedures and perspectives each branch brings to the competition; and the third the key areas of struggle that have marked the recent record, notably the Reagan years. The last part is the meat of the book and where Warburg is at his best. Congress's powers to declare war, approve treaties, control the purse, and otherwise affect through its general legislative powers the form and flow of foreign relations in such seemingly disparate areas as arms sales, human rights, and strategic weapons are explored. The specific issues examined simultaneously catalog many of the most important foreign policy issues of the Reagan era and those that lie at the center of the legislative-executive struggles of the past two decades. Conditionality, micromanagement, and a host of other strategies familiar to congressional scholars become the expression of the policy disagreements in the context of divided government that lie at the core of the struggles. Arguably there is nothing new or distinctive in the treatment here, but that does not detract from its utility and from the contribution it makes to an understanding of the cyclical character of legislative-executive relations.

Warburg's treatment of the counterreformation of the 1980s is written with the flair of a political journalist and is laced with prescriptive judgments that reflect his long experience as a staffer on Capitol Hill and as a political consultant. Inevitably, this sometimes leads to conclusions that are not beyond dispute. That is nowhere more evident than in the concluding chapter, where the author argues that "ideological convergence" (p. 309) and "the wane of ideology" (p. 310) have become characteristic of foreign policy making. The assertions are at odds with a growing number of systematic analyses that document extensive and persistent partisan and ideological divisions in the foreign policy domain comparable to what was long thought to apply only to domestic policy issues. Moreover, as the Soviet empire disintegrates and the anticommunist sentiments that once sustained the myth that politics stop at the water's edge dissipate, partisan and ideological dispute will become more prevalent, not less. Moreover, the conclusion is at odds with much of Warburg's own analysis. Still, the book merits attention as a useful contribution toward understanding an enduring struggle.

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Errata

Vol. 84, No. 3, p. 990. *How Presidents Test Reality: Decisions on Vietnam, 1954 and 1965.* Should be properly attributed to John P. Burke and Fred I. Greenstein with the collaboration of Larry Berman and Richard Immerman. New York: Russell Sage Foundation.

Vol. 84, No. 3, p. 1007. *Elite Cadres and Party Coalitions: Representing the Public in Party Politics.* Should be properly attributed to Denise A. Baer and David Bositis. Westport, CT: Greenwood Press.

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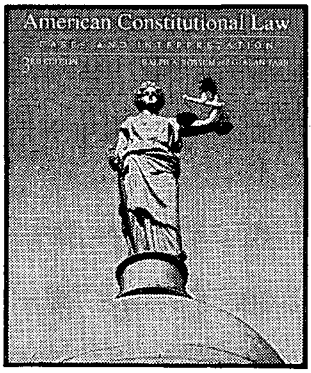

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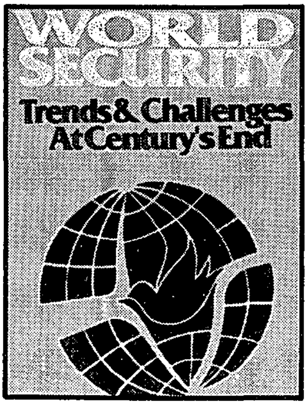

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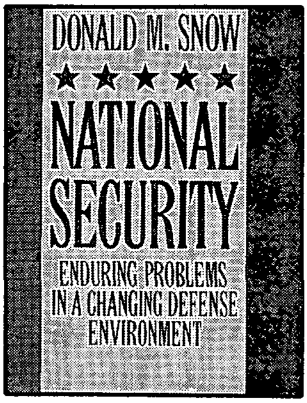
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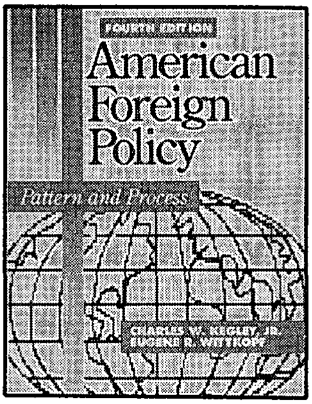
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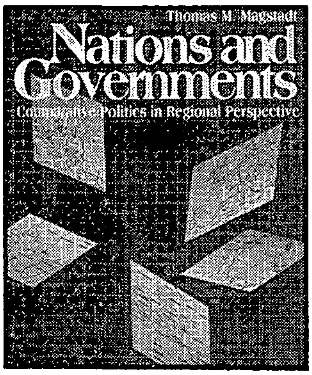

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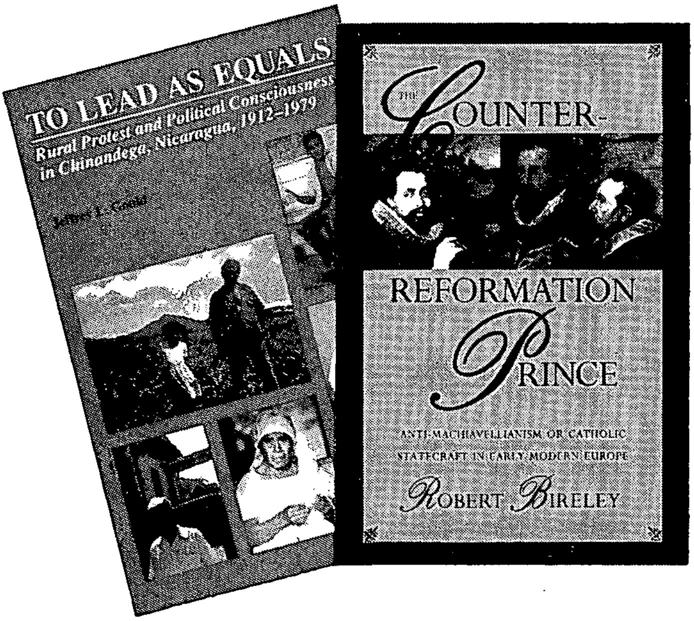
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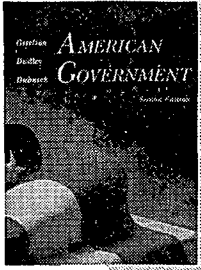

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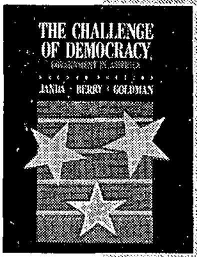

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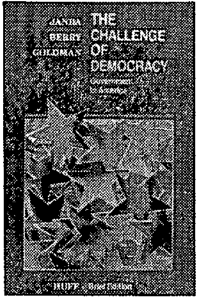
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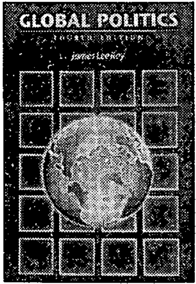
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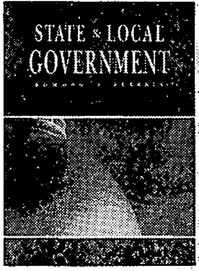
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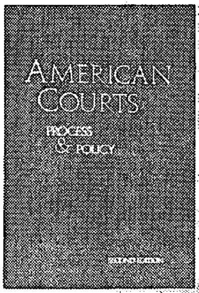
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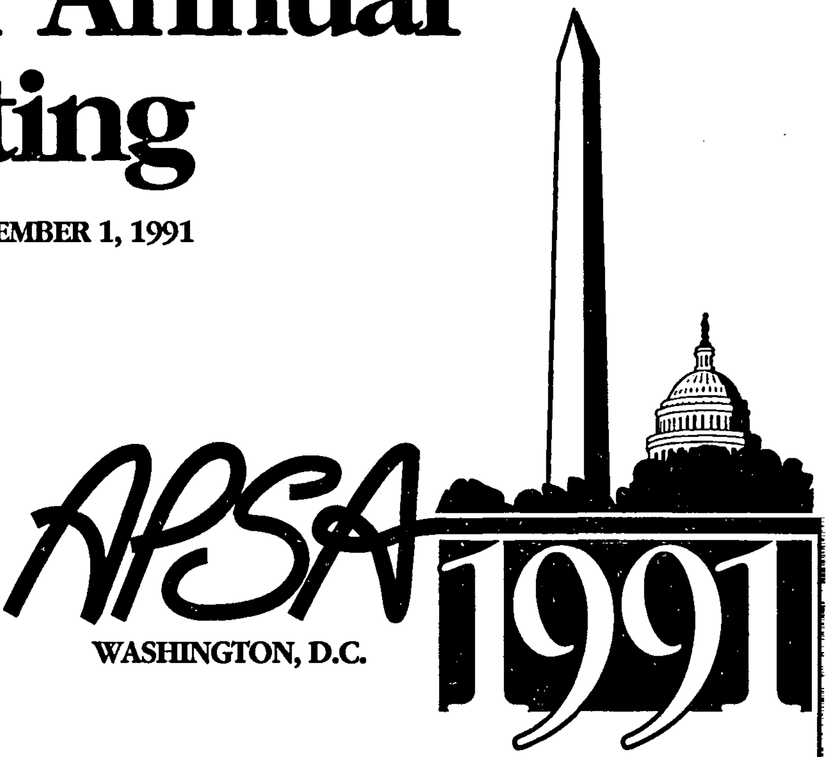

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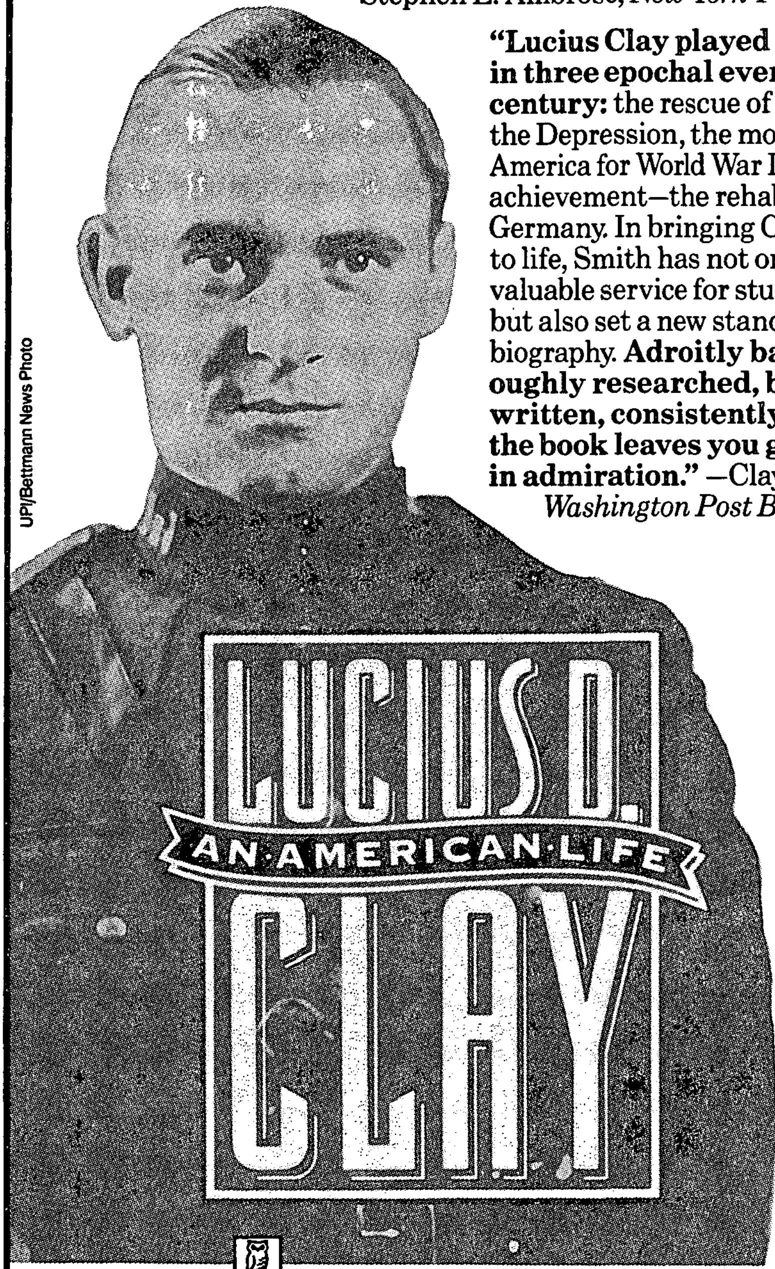

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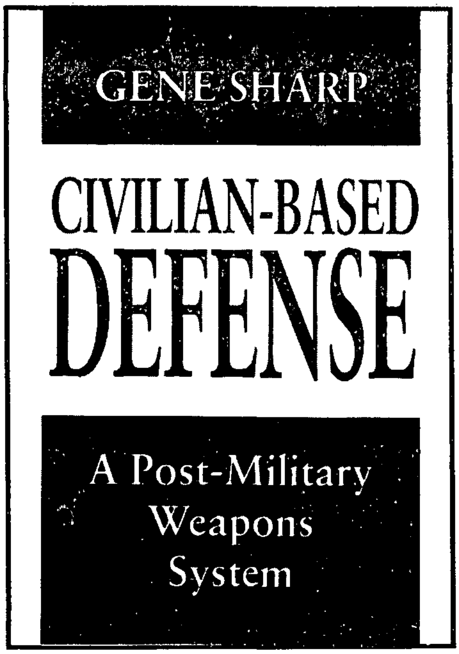

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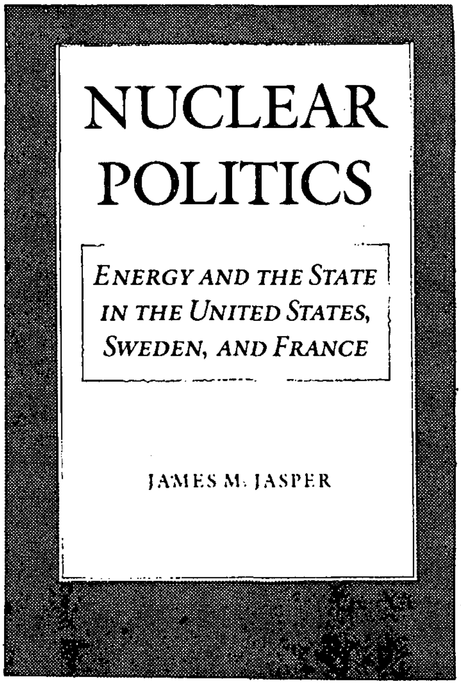

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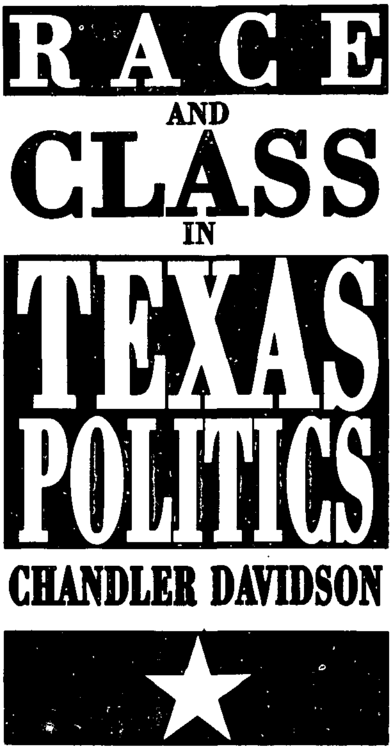
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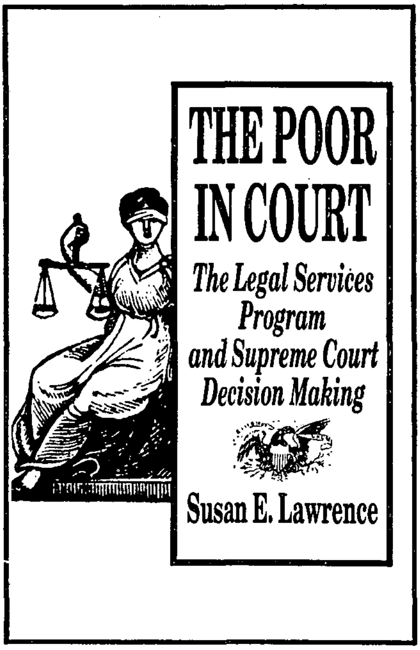
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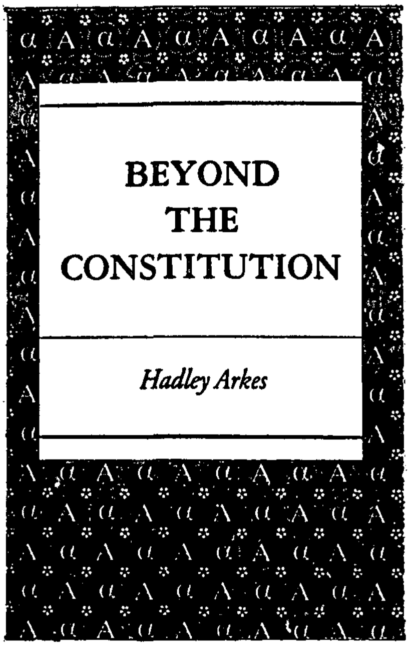
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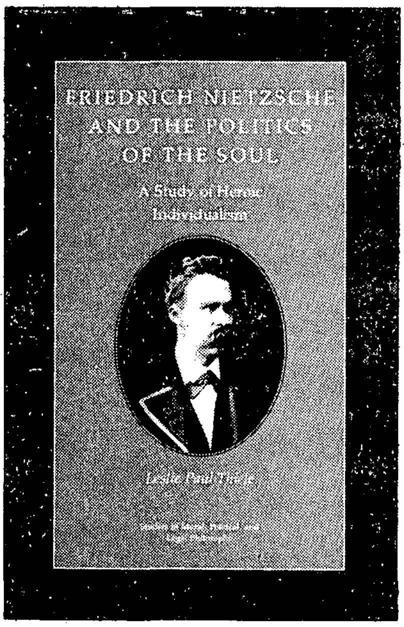
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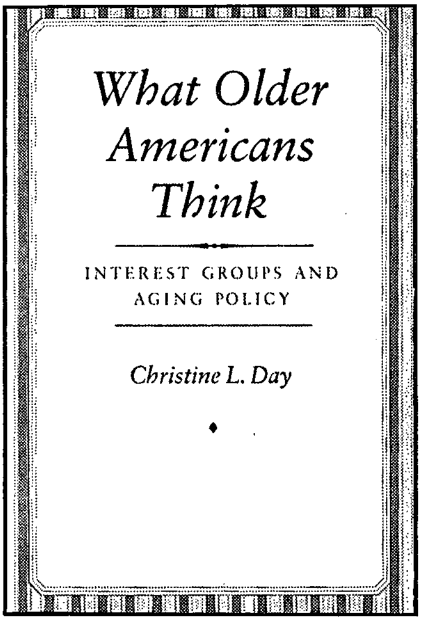
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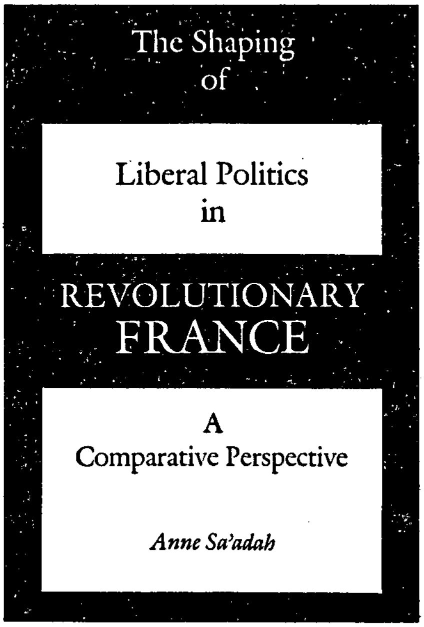
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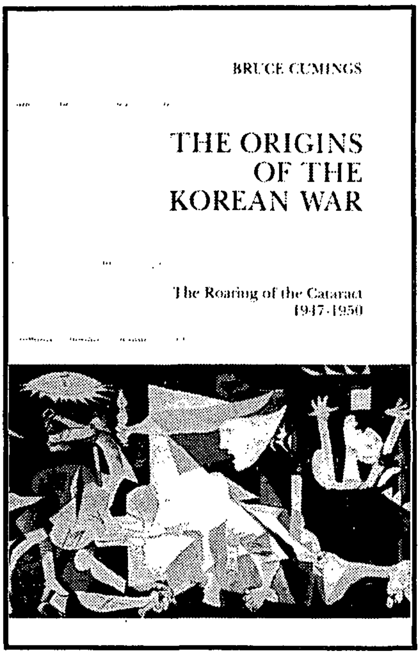

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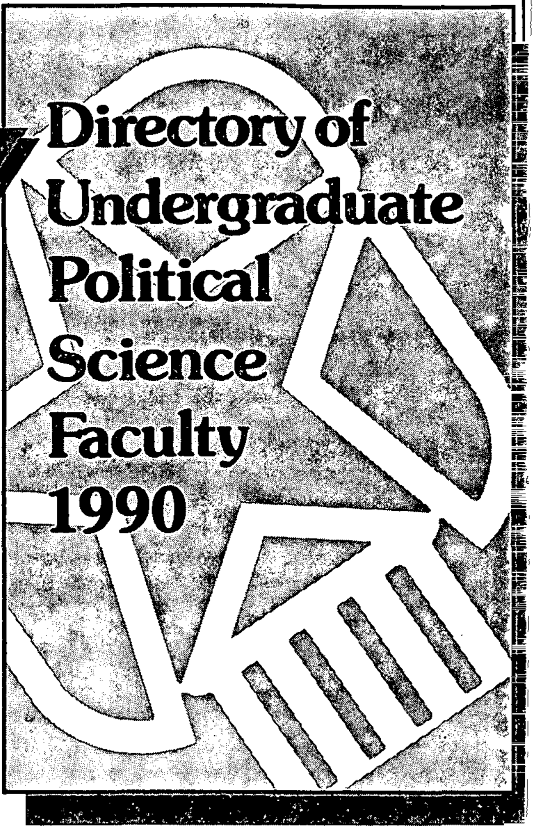

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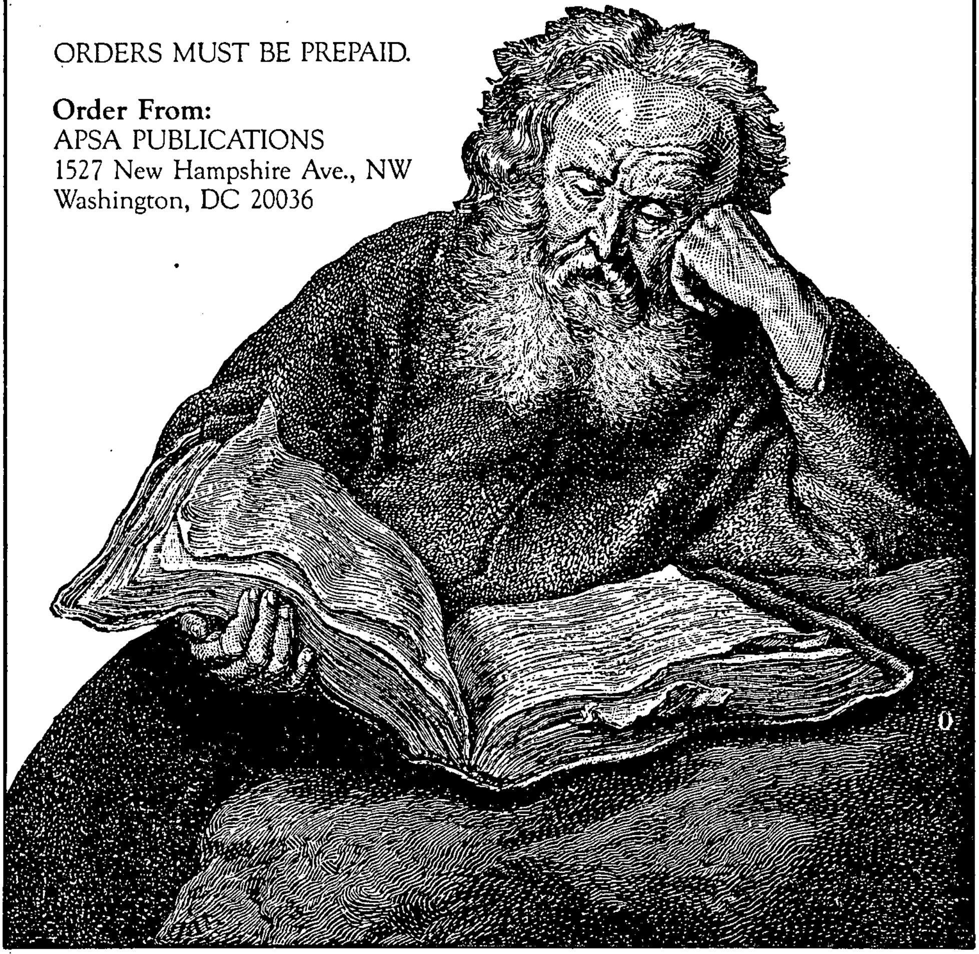

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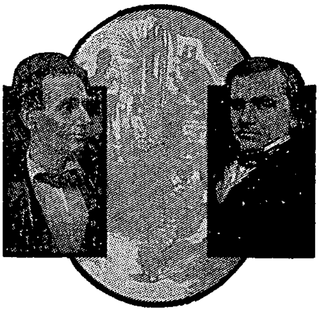

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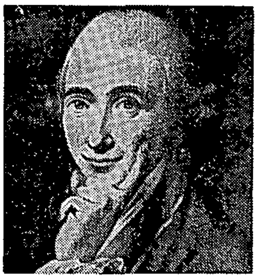

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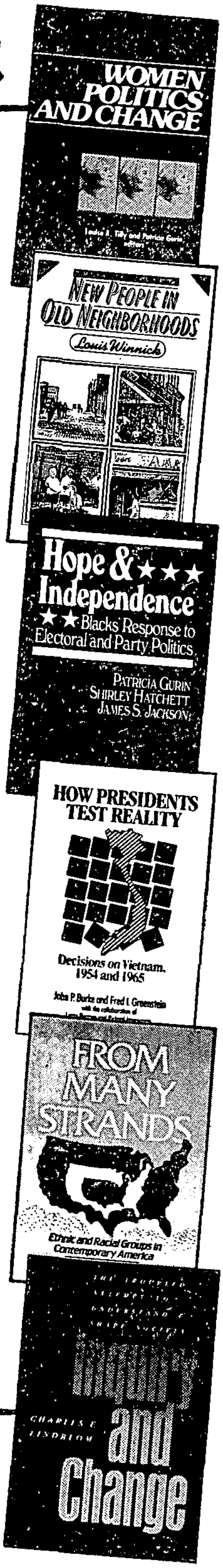
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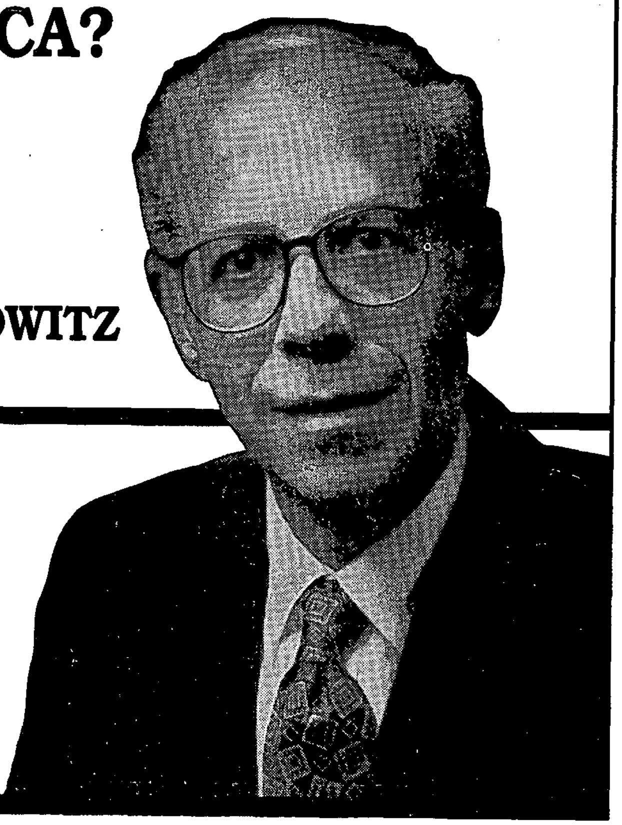

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